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DSSD CENSUS 2000 PROCEDURES AND OPERATIONS MEMORANDUM SERIES B-1*

MEMORANDUM FOR John H. Thompson, Chair
Executive Steering Committee for A.C.E. Policy

From: Howard Hogan, Chief
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Subject: Data and Analysis to Inform the ESCAP Report

The attached document was prepared, per your request, to assist the Executive Steering Committee on A.C.E. Policy (ESCAP) in assessing the data with and without statistical correction.

This report seeks to summarize the data and analysis presented to ESCAP either through the "B-Series," or through discussions and presentations. All results and findings presented in this document are preliminary and subject to verification upon receipt of final data files.

cc: ESCAP

DSSD Census 2000 Procedures and Operations Memorandum Series B-1
March 1, 2001

Accuracy and Coverage Evaluation: Data and Analysis to Inform the ESCAP Report

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Table 1. Summary Table: Data and Analysis to Inform the ESCAP Report

Finding	Evidence	Implication	Limitations[1]
What do we know about Census 2000?			
Census 2000 was similar in design to the 1980 and 1990 censuses	Procedural histories and Census 2000 Operational Plan, 12/00	We expected similar patterns of coverage error.	
Census 2000 included some important improvements such as paid advertising, intensive community outreach, local involvement with address list development, and competitive pay scales.	Census 2000 Operational Plan 12/00	We expected these programs to have a modest impact in reducing the undercount	
While there were some local problems and minor operational shortcomings, Census 2000 was generally well designed and executed.	Report B-3, "Quality of Census Processes"	We expect generally good and uniform patterns of coverage, with perhaps local exceptions.	
There is evidence of geographic heterogeneity in the application of some census processes.	Presentation at ESCAP, 12-20-99, 4-12-00, 5-24-00 and B-14, Feasibility, pp. 46-48	Local heterogeneity will affect the accuracy of both the census and the adjusted estimates.	We cannot know the effects of this differential pattern on census net undercount at the local level.
There was a high level of advertising and outreach targeted at minority populations.	Census 2000 Operational Plan 12/00	We expected these programs to have a modest impact in reducing the undercount.	

[1] All findings are based on the best available evidence as of today; further evaluations could modify them.

Finding	Evidence	Implication	Limitations
What does the A.C.E. tell us about Census 2000?			
The level and patterns of coverage in Census 2000 are substantially similar to those of the past two censuses, with incremental improvements rather than wholesale discontinuities.	Report B-2, "Overall Census and A.C.E. Quality Indicators;" Report B-3, "Quality of Census Processes;" Feasibility Doc.	This finding is consistent with what is known about the design and implementation of Census 2000.	
While Census 2000 reduced both the net and the differential undercount, the A.C.E. estimates that the census undercounted the total population by approximately 1.18 percent and continued previous patterns of differential coverage, with lower coverage rates for minorities, renters, and children.	Report B-9, "Dual System Estimation Results"	A lower undercount in the Census means the benefits from adjusting a loss.	All results are subject to sampling and nonsampling errors.
Was the A.C.E. conducted as designed?			
The A.C.E. was carried out as pre-specified with only minor modifications, which were warranted and documented when important information became available earlier than expected.	Report B-7, "Missing Data Results;" Report B-8, "Decomposition of Dual System Estimate Components;" Report B-9, "Dual System Estimation Results"	The results of the A.C.E. were not manipulated.	There were two changes to prespecification, one concerning the collapsing rules and the other affecting the missing data imputation.
Was the A.C.E. an operational success?			
The A.C.E. was an operational success; listing, interviewing, matching, and follow-up were all conducted as designed and were well controlled.	Report B-5, "Person Interviewing Results;" Report B-6, "Person Matching and Followup Results;" Report B-7, "Missing Data Results"	There were no unforseen operational difficulties with a significant effect on the quality of the data.	

Finding	Evidence	Implication	Limitations
The A.C.E. significantly reduced sampling variance.	Report B-9, "Dual System Estimation Results;" Report B-11, "Variance Estimates by Size of Geographic Area"	There were no unforseen operational difficulties with a significant effect on the quality of the data.	
Consistent reporting of Census Day addresses may have been somewhat better than that achieved in 1990 due to better interviews made possible by the Computer Assisted Personal Interviewing instrument. Interviewing was conducted closer to Census Day.	Report B-6, "Person Matching and Followup Results"	Data collection error probably lower than in 1990.	
Matching error in the A.C.E. was low, with indications that it is substantially lower than that achieved in 1990.	Report B-6, "Person Matching and Followup Results;" Presentation to ESCAP, 11-30-00 and 2-2-01, Feasibility, p.43	A.C.E. processing errors are probably less than those measured in 1990.	
A.C.E. fabrication was more tightly controlled than in the 1990 PES; tighter field management reduced the opportunity for fabrication.	Report B-6, "Person Matching and Followup Results"	Data collection error probably lower than in 1990.	
The level and pattern of missing data in the A.C.E.'s comparable to that in the 1990 PES.	Report B-7, "Missing Data Results"	Effect on A.C.E. quality is similar to that experienced in the 1990 PES.	

Finding	Evidence	Implication	Limitations
Questions remain concerning the level of balancing error.	Report B-8, "Decomposition of Dual System Estimate Components;" Minutes, Feasibility p. 50, Report B-18 , "Effect of Targeted Extended Search"	Increased balancing error could make the adjustment less accurate.	A full analysis has not been completed.
E-Sample coding errors were controlled comparable to 1990, except, perhaps, for e-sample geocoding.	Series T-6: "Additional Geographic Coding for Erroneously Enumerated Housing Units"	A.C.E. might over estimate the census undercount.	There was evidence of some A.C.E. mis-geocoding.
Correlation bias is almost certainly present for both Black and non-Black populations. The switch to PES-C may have increased correlation bias over 1990 levels, but the evidence on the level of correlation bias is weak.	Report B-4, "Demographic Analysis Results;" Report B-12, "Correlation Bias;" Presentation to ESCAP, 7-12-00, Feasibility pp. 35-36	A.C.E. could underestimate the undercount.	Limited data on females, children etc.
The A.C.E. contains bias due to synthetic estimation.	Report B-14, "Synthetic Assumptions,"	The A.C.E. will not remove local variations in the net undercount that are not correlated with the poststrata.	Only indirect evidence is available.

Finding	Evidence	Implication	Limitations
What does Demographic Analysis say about the census?			
Initial Demographic Analysis estimates indicate a net census overcount of 0.7 percent with large overcounts for the non-Black population age 18-29	Report B-4, "Demographic Analysis Results"	The level and pattern of the A.C.E. estimates differs from the initial Demographic Analysis estimates.	The Demographic Analysis estimates are subject to their own patterns of uncertainities.
Alternate Demographic Analysis benchmarks indicate a net undercount of 0.9 million, or 0.32 percent.		The A.C.E. may be overestimating the population size.	The Demographic Analysis estimates are subject to their own patterns of uncertainities.
Both the initial and revised Demographic Analysis indicates an improvement in coverage from the 1990 to the 2000 censuses	B-4, "Demographic Analysis Results"	Census 2000 net coverage is higher than 1990.	
Both the initial and the revised Demographic Analysis indicate a differential undercount in Census 2000	B-4, "Demographic Analysis Results"	Census 2000 did not eliminate the differential undercount.	
What does loss function analysis tell us about the relative accuracy of the adjusted and unadjusted census?			
If there is little or no correlation bias and the level of A.C.E. errors is the same as the 1990 PES, the A.C.E. is less accurate than the census.	Report B-13, "Comparing Accuracy"	If these conditions are true, the census is probably the more accurate.	These results are dependent on the model assumptions being approximately true.

Finding	Evidence	Implication	Limitations
If there is moderate correlation bias or if the level of A.C.E. processing errors is substantially reduced, the A.C.E. is more accurate.	B-13, "Comparing Accuracy"	If these conditions are true, the adjusted figures are probably the more accurate.	These results are dependent on the model assumptions being approximately true.
Accounting for local census heterogeneity is unlikely to reverse the findings for the loss function analysis.	B-14, "Synthetic Error"	Heterogeneity is a concern but probably not a deciding factor.	Measuring the effect of local variation is dependent on finding observable variables that have similar geographic distributions as the net undercount.
What does an analysis of the A.C.E./PES error of closure tell us about the level and pattern of DSE errors?			
The level and pattern of errors in the A.C.E. may differ from that of the 1990 PES.	B-13, "Comparing Accuracy" B-14, "Synthetic Error," and "Overview of Total Error Modeling and Loss Function Analysis	The findings from the loss function analysis, which depend upon an assumption of A.C.E./PES similarity in error structure, may be misleading.	This result depends upon Demographic Analysis's ability to place an upper bound on the level of population change between 1990 and 2000.

Accuracy and Coverage Evaluation: Accuracy and Coverage Evaluation: Data and Analysis to Inform the ESCAP Report

prepared by Howard Hogan

Introduction

Background

The Census Bureau designed the Accuracy and Coverage Evaluation (A.C.E.) to permit correction of the initial census results to account for systematic patterns of net undercount. The Census Bureau preliminarily determined that the A.C.E., if properly conducted, should produce more accurate census data by improving coverage and reducing differential undercounts; the purpose of this document is to evaluate whether the data produced in Census 2000 support this initial determination.

This document summarizes and synthesizes the more detailed analysis reports that were written to inform the adjustment decision. No one analysis report is determinative; rather the information in the analysis reports, taken together, permits evaluation of the quality of both the census and the A.C.E. The topics of the analysis reports were selected because the Census Bureau believed that the information in those reports would provide the basis for informing the Census Bureau's adjustment decision. In the course of evaluating the conduct of both the census and the A.C.E., it became evident that other analyses should be completed; thus, two additional reports have been added to the 16 formal reports originally specified. The information in the analysis reports, and the reports themselves in draft form, have been shared with the Executive Steering Committee on A.C.E. Policy (ESCAP) on a flow basis so that the Committee could evaluate the data as they became available. The Committee has sometimes asked for additional information, either from the authors of the analysis reports or from other Census Bureau staff. Much of the analysis in the attached reports is applicable for all possible uses of adjusted data, but in some instances the reports focus on the ESCAP Committee's initial regulatory charge: to make a recommendation on the suitability of using the A.C.E. data for redistricting.

As this document is written, the ESCAP is in the process of evaluating which set of numbers, the adjusted or the unadjusted, is more accurate for redistricting purposes. If more than one set of numbers is available, each of the 50 states will then make its own decision on which set of data to use. The Census Bureau believes it is appropriate for it to make one determination on which set of data is more accurate, rather than 50 separate determinations, because the statistical determination of the relative accuracy of the census versus the A.C.E. results is meaningful when summarized across jurisdictions. However, we have not attempted, nor do we think it possible, to establish the relative accuracy of a particular state.

The authors of the attached reports have analyzed the best data available at this time. It should be noted that in the years following Census 2000, as in past censuses, the Census Bureau will prepare an extensive array of detailed evaluations of many aspects of both the initial census and the A.C.E. A list of the evaluation categories and their projected completion dates is attached. These evaluations will not be available for months, or in some cases, years, after the Census Bureau is required by law to provide redistricting data to the states. These final evaluations, as distinguished from these analytical reports that inform the ESCAP Committee, will be accomplished without the pressure of a legal deadline, will be based on additional information, and may, in some instances, reach conclusions different from those in certain of these reports.

Accuracy and Coverage Evaluation (A.C.E.) Dual System Estimates

The A.C.E. indicates that Census 2000 reduced both net and differential undercoverage over the levels measured by the 1990 Post-Enumeration Survey (PES). The net national undercount is estimated to have been reduced from the 1990 rate of 1.61 percent (0.20 percent standard error) to 1.18 percent (0.13 percent standard error). The estimated undercount rate for the Non-Hispanic Blacks domain dropped from 4.57 percent (0.55 percent standard error) to 2.17 percent (0.35 percent standard error), and the estimated undercount rate for the Hispanics domain dropped similarly from 4.99 percent (0.82 percent standard error) to 2.85 percent (0.38 percent standard error). In addition, the estimated undercount rate for children dropped from 3.18 percent (0.29 percent standard error) to 1.54 percent (0.19 percent standard error). (Report B-9, "Dual System Estimation Results")

Nonetheless, the improvements demonstrated in Census 2000 do not mean that complete coverage has been achieved or that differential coverage has been eliminated. On the contrary, the A.C.E. indicates that Census 2000 perpetuated longstanding patterns of differential coverage, with minority groups and children exhibiting lower coverage rates. The Census 2000 percent net undercount for the non-Hispanic Black and the Hispanic domains, 2.17 percent and 2.85 percent respectively, remain significant, as does the Census 2000 percent net undercount for children of 1.54 percent.

Tenure continues to be an important characteristic to distinguish coverage. The A.C.E. indicates that the pattern of differential coverage continues despite improvements in Census 2000. The percent net undercount for non-owners was 2.75 percent (0.26 standard error) as compared with an estimated net undercount for owners of 0.44 percent (0.14 standard error). While this is a distinct improvement over the percent net undercount for non-owners in the 1990 census, which is estimated at 4.51 percent (0.43 standard error), the A.C.E. indicates that the estimated undercount for this population is significant as well.

In addition, the undercount for minority renters also remains high. The non-owner undercount for non-Hispanic Blacks was estimated to be 3.58 (0.48 standard error), for Hispanics 4.32 (0.55 standard error), for Asians 1.58 (0.98 standard error), for Hawaiians and Pacific Islanders 6.58 (4.07 standard error), and for American Indians not on reservations 5.57 (2.02 standard error).

Tables 2a and 2b provide the percent net undercount for the race/origin domains, tenure, and age/sex groups for Census 2000 and the 1990 census.

Table 2a: Percent Net Undercount for Major Groups: 2000 A.C.E.

Estimation Grouping	Net Undercount (percent)	Standard Error (percent)
Total population in Households	1.18	0.13
RACE AND HISPANIC ORIGIN		
American Indian and Alaska Native (on reservation)	4.74	1.20
American Indian and Alaska Native (off reservation)	3.28	1.33
Hispanic Origin (of any race)	2.85	0.38
Black or African American (not Hispanic)	2.17	0.35
Native Hawaiian and Other Pacific Islander (not Hispanic)	4.60	2.77
Asian (not Hispanic)	0.96	0.64
White or Some Other Race (not Hispanic)	0.67	0.14
AGE AND SEX		
Under 18 years	1.54	0.19
18 to 29 years		
Male	3.77	0.32
Female	2.23	0.29
30 to 49 years		
Male	1.86	0.19
Female	0.96	0.17
50 years and over		
Male	-0.25	0.18
Female	-0.79	0.17
HOUSING TENURE		
In owner-occupied housing units	0.44	0.14
In nonowner-occupied units	2.75	0.26

Table 2b: Percent Net Undercount for Major Groups: 1990 PES

Estimation Grouping	Net Undercount (percent)	Standard Error (percent)
Total Population 1/	1.61	0.20
RACE AND HISPANIC ORIGIN		
White or Some Other Race (not Hispanic) 2/	0.68	0.22
Black or African American	4.57	0.55
Hispanic Origin 3/	4.99	0.82
Asian and Pacific Islander	2.36	1.39
American Indian and Alaska Native (on reservation)	12.22	5.29
AGE AND SEX		
Under 18 years	3.18	0.29
18 to 29 years		
Male	3.30	0.54
Female	2.83	0.47
30 to 49 years		
Male	1.89	0.32
Female	0.88	0.25
50 years and over		
Male	-0.59	0.34
Female	-1.24	0.29
HOUSING TENURE		
In owner-occupied housing units	0.04	0.21
In nonowner-occupied housing units	4.51	0.43

Review of the Quality of the Census Operations

Conclusions for this section

While many elements of the design of Census 2000 were fundamentally similar to the 1990 census, there were numerous major changes. These included involving local governments in the address list building process, increasing methods for answering the census, designing a simplified questionnaire, developing a multi-step mailing strategy, creating a paid advertising campaign, and restructuring the pay scale for temporary workers. The paid advertising campaign (over $100 million dollars) allowed for a saturation of census awareness across the nation, particularly for the minority communities. The restructured pay scale meant that the census could compete successfully with other employers to hire the number and quality of field workers it needed to conduct the census well.

Operationally, Census 2000 was a success. The census data collection was accomplished on schedule with only a few exceptions. A review of the evidence from field reports and quality assurance processes indicates that Census 2000 programs functioned effectively within design parameters.

Analysis reports important to this section

(All Analysis Reports cited in the text are in the DSSD Census 2000 Procedures and Operations Memorandum Series B)

- Report B-2: "Quality Indicators of Census 2000 and the Accuracy and Coverage Evaluation," by James Farber

- Report B- 3: "Quality of Census 2000 Processes," by James B. Treat, Nicholas S. Alberti, Jennifer W. Reichert et al.

Discussion

As documented extensively by Census Bureau and outside statisticians, every census since at least 1940 has experienced both a net undercount and a substantial differential undercount. In particular, the data reveal a persistent differential undercount between the Black and non-Black populations, as well as differential undercounts for other minority groups and for children.

Many elements of the design of Census 2000 were fundamentally similar to the design of the 1990 census. Address lists were prepared using a variety of sources, and questionnaires were delivered to each address on the list. Questionnaires were principally delivered by the U.S. Postal Service; however, in areas with rural style addresses, census workers delivered the questionnaires. Households receiving questionnaires were asked to return the questionnaires by mail, although in some very rural or isolated areas households were interviewed by census enumerators as the enumerators verified and updated the address list. Those addresses that did not return a questionnaire by mail were followed up by census workers in the Nonresponse Follow-up operation (NRFU). NRFU was followed by special coverage improvement follow-up operations, which, among other things, included contacting addresses listed as vacant or nonexistent by the NRFU field staff. Each of these operations had its own quality control procedures.

The Census 2000 plan, however, included several important innovations to the census process designed to improve census accuracy. Prior to Census 2000, the Census Bureau worked closely with local and tribal governments through the Local Update of Census Addresses (LUCA) program to review and update the address list. During LUCA, local and tribal government officials were given the opportunity to review the Census Bureau's address list and identify missing addresses for inclusion in the census. The Census Bureau also implemented the New Construction Program, during which local governments were invited to submit addresses for housing units that had been built subsequent to the completion of the address list in January 2000. The "Be Counted" program was also new in Census 2000. "Be Counted" forms were provided to individuals who believed that they might have been missed in the initial distribution of census questionnaires, as well as to individuals without any usual residence. The "Be Counted" forms were made available to the public at walk-in Census 2000 assistance centers and at a variety of public locations identified through consultation with local organizations. In addition, Census 2000 questionnaires were available upon request in six languages and language assistance guides were available in more than forty languages. Households also were given the opportunity to respond to Census 2000 by telephone or via the Internet.

To encourage households to respond to Census 2000, the Census Bureau initiated the largest promotion and outreach effort in its history for a decennial census. The Census Bureau established approximately 140,000 partnerships with a wide range of government and nongovernment organizations at the national and local levels. Organizations throughout the United States and Puerto Rico implemented promotional activities to educate the public about the importance of participating in the census. Then, starting in November 1999, the Census Bureau launched the first-ever paid advertising campaign for a census. This campaign was extended in targeted cities to encourage cooperation with enumerators during the NRFU operation. Other efforts included the distribution of numerous news releases and a number of video news feeds tailored to local areas to media outlets to generate media coverage during the various stages of Census 2000.

The Census Bureau then implemented the A.C.E. because it expected that, while these innovations would improve the results of the census, the phenomenon of the differential undercount would continue. The A.C.E. is designed to serve as a quality check on the census counts obtained after all other operations planned for Census 2000 were completed. In effect, the goal of the A.C.E. is to make a good census even better.

The discussion in this document is not meant to be a complete evaluation of census operations, but rather focuses on information relevant to the question of the level and pattern of census omissions or erroneous inclusions, because this information is directly relevant to understanding and assessing the results of the A.C.E.

We will discuss what is known about the following major operations:

- Address List Development
- Questionnaire Delivery and Return
- Nonresponse Follow-up
- The "Be Counted" Campaign
- Coverage Edit Follow-up
- Coverage Improvement Follow-up
- Housing Unit Duplication Operation[2]
- Primary Selection Algorithm
- Unclassified Unit and Missing Data Estimation

Address List Development

Address list development was conducted over several years, and the vast majority (96.7 percent) of addresses were listed before questionnaire delivery. One major change from previous censuses was the inclusion of the Local Update of Census Addresses (LUCA) program, during which the Census Bureau solicited the help of local governments in the address list building operation. LUCA was successful in adding approximately five million housing units to the address list[3]. However, anecdotal evidence suggests that the LUCA program may also have contributed duplicate addresses to the Master Address File (MAF). Duplicate addresses may have been erroneously added because the Census Bureau and local governments refer to the same address in different ways.

The address list development process included several quality assurance programs. These programs had the following objectives: to prevent errors due to lack of knowledge or understanding on the part of the lister, to control coverage and content errors, and to promote continuous improvement of performance. In general, the preliminary quality assurance results for address list development are within the expected range for each of the programs.

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[2]The Housing Unit Duplication Operation was a special operation designed and instituted after the Coverage Improvement Follow-up to reduce the level of housing unit duplication. This operation has special implications for census coverage and the conduct of the A.C.E.

[3]Many of the adds were also added by other operations. At this time, we do not know the extent of the overlap. That is, the five million figure cannot be considered as a net addition.

Questionnaire Delivery and Return

The United States Postal Service or census workers delivered questionnaires to the vast majority of addresses on the address list. As in previous censuses, a certain number of questionnaires were misdelivered. For example, the questionnaire intended for Apartment A might have been delivered to Apartment B and vice versa. We have not quantified the level of questionnaire mis-delivery.

Householders were asked to return the questionnaires by mail. Since the Census Bureau does not expect mail responses from vacant or nonexistent housing units, the relevant measure of cooperation is the return rate, that is, the proportion of occupied housing units that returned their questionnaire. This measure differs from the response rate, which while available earlier in the census process, includes vacant and nonexistent housing units in the denominator. As measured by the return rate, the cooperation of the public with Census 2000 was approximately the same as in the 1990 census. The 2000 return rate (72 percent) is approximately the same as the 1990 return rate (74 percent). The comparison is not exact because the universes are slightly different. Considering the general trend downward in return rates between censuses and for survey interviews in general, the Census Bureau considers the Census 2000 return rate to be a major success.

Nonresponse Follow-up

During Nonresponse Follow-up (NRFU), census workers visited each household that the address list identified as not yet having returned a mail questionnaire. In Census 2000, approximately 42 million households were included in the NRFU process. Thanks in large part to adequate funding provided by Congress, pay rates and levels of staffing in 2000 were far better than in the past two censuses. We believe that this increased funding and the ability to hire adequate staff contributed to an improvement in NRFU quality.

For most LCOs, NRFU was completed as scheduled in a nine-week period between April 27, 2000, and June 26, 2000. This performance compares favorably with 1990, when NRFU was conducted over a 14-week period from April 26 though July 30. The Census Bureau believes that, all other things being equal, NRFU interviews conducted closer to Census Day are likely of higher quality.

Local NRFU problems were identified in a few local census offices, including the local census office in Hialeah, Florida. The Census Bureau responded to the localized problems in the Hialeah office by re-enumerating certain areas that were believed to have faulty data. The Census Bureau does not believe that net coverage in the Hialeah or any other local census office was substantially affected by these local problems; the NRFU operation for the nation as a whole was good to excellent.

The NRFU quality assurance program was conducted through a random and targeted reinterview program which had the following three objectives:

- Prevent errors due to lack of knowledge or understanding
- Control coverage and content errors
- Promote continuous improvement of performance

Preliminary NRFU quality assurance results show that the reinterview workload was 6 percent, slightly above the expected workload of five percent. Discrepant cases were found in approximately three percent of the reinterview cases. Some local census offices experienced delays in starting their reinterview programs, which may have hindered the reinterviewers' ability to accurately verify the census data. A significant number of quality assurance forms were lost and/or completed incorrectly. (Report B-3, "Quality of Census 2000 Processes")

In spite of local imperfections, the NRFU program as a whole was largely successful. The better pay and staffing seemed to have resulted in a more professional and controlled labor force. The local problems and quality assurance shortcomings were similar to problems encountered in previous censuses and should be expected in any nonrecurring operation of this magnitude.

Be Counted Campaign

The "Be Counted" campaign was designed to allow people who thought they may have been missed by the census to send in a "Be Counted" form, listing themselves and their April 1, 2000, address. The Census Bureau had hoped that this campaign would allow for improved cooperation and coverage. The National Academy of Science and others feared that large numbers of "Be Counted" forms would overwhelm the system and lead to increased person duplication.

Neither the hopes nor the fears relating to the "Be Counted" campaign were realized. The Be Counted workload was only approximately 600,000, with no large local clusters observed. Its impact on net coverage for any group or area was minimal, and it is not believed to have contributed to housing unit duplication.

Coverage Edit Follow-up

Under certain circumstances, the Census Bureau would call a responding household on the telephone to gain additional information. This extra effort, called Coverage Edit Follow-up (CEFU), was designed to improve within-household coverage, especially for large households. The census questionnaire had room to collect data for six people and asked the respondent living in a household with more than six people to list the additional residents. In CEFU, enumerators called these households and gathered the required information about the additional residents. In addition, CEFU was designed to follow up count discrepancies, or cases where the population count on the front of the questionnaire differed from the number of person responses inside the questionnaire.

Due to computer problems, the start of CEFU was delayed until May 8, 2000. It ran through August 13, 2000. Originally, it was planned for April 5, 2000, through June 19, 2000. This delay may have made it more difficult to obtain good information from households with more than six residents because some of the residents may have moved. In addition, CEFU had no provision to contact large households without telephones. When the Census Bureau could not secure good CEFU data on listed additional residents, it imputed their characteristics; to do otherwise would have decreased net coverage. Thus, the CEFU operation may have resulted in some small coverage loss compared to previous censuses, but this possible loss has not yet been quantified and is not expected to be significant, given the use of imputation.

Coverage Improvement Follow-up

Coverage Improvement Follow-up (CIFU) was designed as a check on addresses that were determined during the NRFU operation to be vacant or deleted (nonexistent). CIFU was also used for addresses requiring follow-up that were identified too late to be included in NRFU. CIFU was conducted from June 26 until September 13. Both the 1980 and the 1990 censuses included similar operations.

CIFU was conducted on 6.5 million addresses for which the housing unit was listed as vacant or non existent in NRFU. CIFU determined that 1.5 million of these units were, in fact, occupied. In addition, CIFU included 2.2 million other addresses that had been added to the MAF after the initial mail out, such as those that resulted from the New Construction or Update/Leave programs.

The quality assurance procedures on CIFU included a questionnaire review, a dependent review and data entry quality assurance. The dependent review was conducted on housing units identified as vacant or nonexistent and excluded certain occupied units for time and budgetary considerations. Some districts may have had a difficult time completing all of the dependent reviews. A significant number of quality assurance forms were lost and/or completed incorrectly. These lost/incorrect forms make any analysis of outgoing quality difficult. (Report B-3, "Quality of Census 2000 Processes")

Housing Unit Duplication Operation

The Census Bureau observed tentative indications as the census progressed that the MAF might contain a significant number of duplicate addresses. The Census Bureau also concluded that the Hundred Percent Census Unedited File (HCUF) might contain a significant number of duplicated persons, many of which are assigned to duplicated addresses. The Census Bureau responded to this problem by designing and conducting the Housing Unit Duplication Operation (HUDO). While this program was not prespecified, the Census Bureau believed that failure to address this potential problem could impair the accuracy of the apportionment numbers. Using the results of an address matching operation and a person matching operation, 2,411,743 address listings (address ID's) were analyzed on an aggregate basis to see whether these addresses were likely to correspond to other addresses already contained in the listing. Based on this analysis, 1,392,686 address IDs were permanently removed from the HCUF; after further review to identify units that may have been removed in error, the remaining 1,019,057 addresses were reinstated and included in the census results. The HUDO was designed solely to remove address/housing unit duplication. The software used for this process was carefully checked.

Primary Selection Algorithm

Census questionnaires contain a unique ID, an identifier that the Census Bureau uses to make sure it records the information for each household only once. Nonetheless, the Census Bureau sometimes receives more than one questionnaire for a single address ID. For example, a household might mail back its questionnaire after the Census Bureau had already created NRFU assignment lists; a NRFU interviewer would then get an interview for a household that had already mailed back its response. As a further example, a "Be Counted" form might be received for a household with a completed census questionnaire. Since NRFU households identified as vacant are sent to CIFU, sometimes multiple questionnaires are generated by design. That is to say that we expect to have one questionnaire from NRFU and another from CIFU. The Primary Selection Algorithm (PSA) examines these multiple questionnaires to form one household to represent the housing unit in the census, sometimes by combining information from more than one questionnaire. The PSA was designed to prevent both overcoverage (including people more than once) and undercoverage (deleting too many people).

Multiple returns were received for less than 10 percent of the address IDs. However, many of these multiple returns were from vacant units or multiple listings of the same people on two IDs. The number of people in a household was found to be larger than the number reported on the most complete questionnaire for fewer than 300,000 IDs. In other words, PSA resulted in an increase of individuals in fewer than 300,000 housing units.

Although no formal evaluation has been completed, the PSA was well programmed and well tested. The results are consistent with the overall design of the PSA and of the census.

Unclassified Unit and Missing Data Estimation

As in the past, Census 2000 had some housing unit records listed on the MAF for which the Census Bureau could not gain information. In addition, there were a small number of housing units which the Census Bureau knows to be occupied but for which it could not secure precise information about the individuals living in that unit. The census process could not always determine whether other units are occupied or vacant. Sometimes, the unit was determined to be occupied, but the number of residents could not be determined. In each of these cases, a statistical process known as "imputation" is used to estimate the number of people living in these units.

Preliminary results indicate that almost 0.4 percent of person records were in housing units on the preliminary HCUF were missing a status of occupied, vacant or nonexistent, indicating that the residents of the housing unit were imputed. For states, the imputation percent ranged from 0.2 percent to 1.1 percent. In 1990, about 0.02 percent of people in unclassified units were imputed.

In addition, Census 2000 encountered whole households where the number of people could be determined, but the person records for these residents were missing. In accordance with past practice, the Census Bureau used imputation techniques to estimate characteristics for these people. About 0.8 percent were imputed with this technique.

The total person substituted persons in the Census 2000 is approximately 1.3 percent. The percent of substituted persons in 1990 was only about 0.7 percent.

Review of A.C.E. Operations

Similar to its review of the operations in the initial census, the Census Bureau has reviewed the A.C.E. operations to identify any deviations from specified procedures and to assess the extent to which the operations were under management control.

Proper execution of the steps between processing and estimation

Conclusions for this section

The A.C.E. was carried out as designed, with only minor modifications. Each modification was well documented and justified by good statistical practice. No steps were skipped because of lack of time or resources, and there was no manipulation of the results or distortions resulting from outside pressures. There is a clear and traceable path from the data collected by the interviewer to the final results. The Census Bureau carried out the A.C.E. according to its public plan, and the steps between processing and estimation were properly executed.

Analysis reports important to this section

- Report B-7: "Accuracy and Coverage Evaluation Survey: Missing Data Results," by Patrick J. Cantwell
- Report B-8: "Accuracy and Coverage Evaluation Survey: Decomposition of Dual System Estimate Components," by Thomas Mule
- Report B-9: "Accuracy and Coverage Evaluation Survey: Dual System Estimation Results," by Peter P. Davis

Discussion

The A.C.E. methodology planned for Census 2000 involves comparing (matching) the information from an independent sample survey to initial census records. In this process, the Census Bureau conducts field interviewing and computerized and clerical matching of the records. Using the results of this matching, the Census Bureau applies the statistical methodology of Dual System Estimation (DSE) to develop coverage correction factors for various population groups. The results are then applied to the census files to produce the adjusted census data.

One concern sometimes expressed about statistical correction is that statistical processes could be subject to manipulation. The Census Bureau believes that this notion is not well founded. The A.C.E. was publicly prespecified to assuage these concerns. The first step in reviewing the A.C.E. is to evaluate A.C.E. operations to determine whether the prespecified procedures were followed and documented. The Census Bureau's analysis found that all planned A.C.E. operations were carried out in close adherence to the prespecified design, with the two exceptions noted below.

The supporting analysis reports review each of the steps in the A.C.E. operation from the creation of the A.C.E. micro-records to the computation of the final adjustment factors. In particular, Report B-8, "Decomposition of Dual System Estimate Components," presents an accounting of the A.C.E. estimation components so that the results can be independently verified. Beginning with records with complete data (meaning records with both post-stratification variables and enumeration status) the accounting then proceeds through each stage of missing data adjustment and sample weighting until the final weighted "matched" results are provided (which are the results that are the input data for the dual system estimates). Report B-8 allows an informed reader to see clearly how the final results were derived and to understand the relative effect of the estimation steps on the results.

Report B-7, "Missing Data Results," shows in detail the effects of individual missing data estimation steps upon the weighted matching results. Report B-9, "Dual System Estimation Results," provides detailed DSE computations together with useful "roll-ups" that aggregate the results by age and sex, minority/nonminority, or other useful summations. This document allows the reader to verify how the final coverage correction factors are computed from the input data.

These three documents, taken together, demonstrate how the final coverage correction factors were derived from the micro-level data and document that the prespecified procedures were followed, with the following exceptions.

The following two changes from the prespecified procedures arose from the unexpected availability of important information in time to improve the A.C.E. estimation:

- The A.C.E. plan provided that cells could be collapsed because of cell size but did not explicitly include variance as a reason for collapsing. We modified these rules because the estimated variance for one cell was unusually large. The design had not anticipated having variance estimates available in time to permit their use in collapsing. When the variances became available earlier than anticipated, the Census Bureau's statistical staff determined the collapsing of "outlier" poststrata was appropriate. This change did not deviate from the purpose or spirit of the prespecified collapsing rules but allowed a more precise application. The change was discussed with the ESCAP and documented.

- Our method for imputing unresolved match and residency status, namely imputation cell estimation, was modified because the results of the A.C.E. follow-up forms became available during the missing data estimation process (Report B-7, "Person Matching and Follow-up Results"). The prespecified design had not anticipated that these data would be available in time to be used in missing data estimation. Analysis of the data indicated that some cases grouped together in the initial missing data design could be separated based on the keyed follow-up results, allowing for a more precise imputation. This change is consistent with normal statistical practice and was discussed with the ESCAP and documented.

Conduct and control of the A.C.E. operations

Conclusions for this section

The A.C.E. was an operational success; it was properly conducted and encountered no unanticipated difficulties. Listing, interviewing, matching, and follow-up were all conducted as designed and were all in control.[4]

Analysis reports important to this section

- Report B-5: "Accuracy and Coverage Evaluation Survey: Person Interviewing Results," by Rosemary L. Byrne, Lynn Imel, and Phawn Stallone

[4]A more extensive description of the A.C.E. can be found in Howard Hogan's paper, "Accuracy and Coverage Evaluation: Theory and Application," prepared for the February 2-3, 2000, DSE Workshop of the National Academy of Sciences Panel to Review the 2000 Census; and Danny R. Childers and Deborah A. Fenstermaker, "Accuracy and Coverage Evaluation: Overview of Design," DSSD Census Procedures and Operations Memorandum Series S-DT-02, U.S. Census Bureau, Washington, D.C., January 11, 2000.

- Report B-6: "Accuracy and Coverage Evaluation Survey: Person Matching and Follow-up Results," by Danny R. Childers, Rosemary L. Byrne, Tamara S. Admas, and Roxanne Feldpausch

- Report B-7: "Accuracy and Coverage Evaluation Survey: Missing Data Results," by Patrick J. Cantwell

Discussion

The second aspect to this review is to establish that the A.C.E. operations were well conducted and well controlled. Reports B-5, "Person Interviewing Results," B-6, "Person Matching and Follow-up Results," and B-7, "Missing Data Results," taken together, establish that the operational quality of the A.C.E. was generally good and that the prespecified design was well followed.

Interviewing

One change from 1990 was the introduction of telephone interviewing. The Census Bureau implemented a telephone program to enhance the efficiency and quality of the A.C.E. interview. The Census Bureau believed that shortening the elapsed time from Census Day to the A.C.E. enumeration would improve data quality and that beginning interviewing early in a more easily controlled environment would allow the A.C.E. supervisors to gain valuable experience in conducting interviews and in operating their laptop computers before training the enumerators. The Census Bureau designed this process to maintain the independence between the A.C.E. and the other Census 2000 operations.

A.C.E. interviewing was an operational success. The A.C.E. interviewing finished on schedule by September 1, 2000, in every local census office except the Hialeah office, where census NRFU interviewing finished late (September 11, 2000) due to local difficulties. There were no major disruptions or delays introduced by the Computer Assisted Personal Interviewing (CAPI) instrument. The timely interviews allowed the Census Bureau to have an orderly completion of interviewing was a major accomplishment.

Twenty-nine percent of the total A.C.E. workload was completed during the telephone phase (April 24 through June 13). These A.C.E. interviews were conducted much closer to Census Day (April 1) than had been possible in 1990, thereby reducing recall bias (the phenomenon of a respondent not remembering the actual situation several months earlier). By design, the telephone phase was restricted to a limited universe of households that were deemed unlikely to have any exposure to continuing census operations. These were primarily households that had mailed back their questionnaires, that had included a telephone number on the questionnaire, and that did not live in certain multi-unit or rural structures. The Census Bureau's conservative use of this interview mode meant that more than 99 percent of the telephone cases were classified as complete or partial interviews and were conducted with a household member.

Table 3. Distribution of Interviews by Week - Unweighted

Phase	Week Starting On	Number of Cases	Cumulative Percent of Person Interviewing Workload
Telephone	April 23, 2000	7,699	2.6
	Apnl 30, 2000	20,590	9.4
	May 7, 2000	25,638	17.9
	May 14, 2000	19,728	24.5
	May 21, 2000	10,497	28.0
	May 28, 2000	3,232	29.1
	June 4, 2000	1,154	29.5
	June 11, 2000	35	29.5
Personal Visit	June 18, 2000	45,204	44.5
	June 25, 2000	57,241	63.5
	July 2, 2000	41,642	77.3
	July 9, 2000	31,344	87.7
	July 16, 2000	17,038	93.4
	July 23, 2000	7,764	96.0
	July 30, 2000	5,057	97.7
	Aug 6, 2000	3,982	99.0
	Aug 13, 2000	1,756	99.6
	Aug 20, 2000	939	99.9
	Aug 27, 2000	336	100.0
	Sept 3, 2000	36	100.0
	Sept 10, 2000	1	100.0

Source: Accuracy and Coverage Evaluation Survey 2000-- housing unit data collected by the Computer Assisted Personal Interviewing (CAPI) instrument.
Report B-5, "Person Interviewing Results."

The automated Computer Assisted Person Interviewing (CAPI) increased the quality of the data captured in the A.C.E. interviews, as the instrument included data edits to ensure a predetermined quality of data before the interview was considered complete. This was not possible with the paper and pencil 1990 instrument. It insured that the interviewer followed the correct path through the interview. CAPI also allowed quick feedback to the interviewers. The Census Bureau's observations and debriefings indicated that CAPI instilled the interviewers with a sense of professionalism and purpose. Observations also indicated that the use of laptop computers enhanced the respect and cooperation exhibited toward the interviewers by the respondent households thereby leading to improved A.C.E. data quality. However, there were a couple of small problems with the CAPI instrument that had minor impacts on quality.

The Nonresponse Conversion Operation (NRCO) was designed to "convert" nonresponse cases, that is, to obtain A.C.E. information for nonresponding households. On a national basis, the NRCO operation successfully converted 70.8 percent of its cases to complete interviews and 14.1 percent to partial interviews. Only 2.2 percent of the cases finished as refusals.

A.C.E. interview rates were very high. The A.C.E. asked questions about both the household living at the address on Census Day and the current household. Because of this, there are two measures of household nonresponse. The rate for occupied housing units on Census Day was 97.1 percent; on the date of the A.C.E. interview, the rate for occupied housing units was 98.8 percent.

These rates compare favorably to the approximately 98.4 percent (unweighted) in the 1990 Post-Enumeration Survey. The unweighted rates for 2000 were 97.0 and 98.9, respectively. Due to the high rate of response, most of the noninterview adjustment factors were very close to one. Consequently, this operation did not change the final weights very much. This helps to keep down the variance of the survey weights.

Missing data rates for characteristic data were very low, ranging from 1.4 percent to 2.4 percent. Compared to the 1990 PES, the rates of characteristic missing data are slightly higher for the age and sex characteristics and slightly lower for tenure and race. Again, this is indicative of good quality interviewing.

The goal of A.C.E. interviewing quality assurance was to ensure that the interviewers did, in fact, visit the designated households, and to prevent systematic errors caused by of lack of knowledge or understanding. The evidence indicates that the A.C.E. interviewing quality assurance operation was properly implemented and successful. A total of 11.6 percent of the cases were subject to random or targeted quality assurance checks. We assume that the 88.4 percent of the cases not in quality assurance share the favorable error rates of the randomly selected cases (0.13 percent). This may have been reduced further as 171 of the remaining errors were corrected in the targeted QA sample.

Matching and Follow-up

Matching refers to the process of determining whether an individual enumerated in the A.C.E. was the same person as an individual enumerated in the census. The matching and follow-up process also determines whether a census record in the E-Sample[5] was complete and correct. Errors in matching can significantly affect undercount estimates; highly accurate matching and processing are an important component of A.C.E. methodology.

Although neither Secretary Mosbacher nor the Committee on Adjustment of Postcensal Estimates (CAPE) identified matching error as a significant problem with the 1990 PES, the Census Bureau made significant improvements to the matching process in the 2000 A.C.E. design. The A.C.E. computer matched the P-sample to the census using the Census Bureau's Statistical Research Division Record Linkage System, a system that the Census Bureau has been developing, testing and using for nearly two decades. Clerical personnel at a centralized location reviewed records that were not matched by the computer matcher. The Census Bureau utilized an ample staff of over 200 clerks, 46 technicians, and 16 analysts so that each successive level of review could perform quality assurance on the previous level. Higher level staff independently reviewed a sample of each employee's work, a process designed to identify random matching errors. Each of the matching levels improved on the previous level. The clerks matched what the computer could not. The technicians worked on any cases the clerks could not resolve and performed the quality assurance on the clerks' cases. Then the analysts finished any cases the technicians could not resolve and performed quality assurance on the technicians' cases.

The results indicate computer matching of 69.6 percent of the P-sample and 64.4 percent of the E-sample. The computer matcher assigned matches very conservatively. Numerous studies over the years have shown that this operation produces insignificant numbers of false matches. Therefore, all questionable matches, possible matches, and near matches are left for clerical review. All nonmatches were clerically reviewed.

We have quality assurance results only on the quality of the clerical matching in the before follow-up stage and the first three stages of after follow-up. The Census Bureau measures matching quality relative to the results that would be produced by the Census Bureau's most experienced and best trained matchers, the 16 analysts permanently employed by the Census Bureau. The quality of the matching process is further measured in terms of changes made by the next level of review; this process tends to overstate the matching error, as not all changes are the result of erroneous matching. However, given these caveats, the outgoing quality rate (the final match rate) for before follow-up was well more than 99 percent. For after follow-up, the outgoing quality rate was also well more than 99 percent. These rates are calculated based on the

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[5]The E-sample refers to the sample of census data defined person records selected for inclusion in the A.C.E. The P-sample refers to the independent sample of people included in the initial A.C.E. interview.

before follow-up and the after follow-up workload and not on the total number of sample cases, that is, they do not include the cases matched by computer. These rates exceed expectations and are indicative of high quality matching.

Person follow-up is also an important A.C.E. process. The follow-up resolves possible matches and, most importantly, determines which E-sample nonmatches are, nonetheless, correctly enumerated in the census. The person follow-up interviews were conducted either by permanent census field staff or by experienced decennial interviewers and the quality assurance operation was targeted at ensuring that the interview was conducted. Of the randomly selected person follow-up quality assurance cases, 0.45 percent resulted in a discrepancy, that is, only 0.45 percent determined that the person follow-up interview may not have been conducted. We can assume that the remaining 84,843 cases not randomly selected for quality assurance have the same rate of failure or roughly 400 cases total that may have not been conducted. In addition, we corrected 84 of those cases in the targeted samples.

Review of A.C.E. Quality

The review in the previous section established that the A.C.E. was conducted as designed. This section will take the next step and evaluate the quality of the A.C.E. as implemented.

Our review of A.C.E. quality has two aspects. First, we review the available data relating to selected individual components of A.C.E. error. The second part of the A.C.E. quality review synthesizes what is known about the components of error into a few indicators of overall relative accuracy for both the adjusted and the unadjusted census results.

Individual components of A.C.E. quality

Sampling Variance

Conclusions for this section

The A.C.E. significantly reduced sampling variance relative to the 1990 PES. This result was achieved by nearly doubling the sample size coupled with significant improvements in the sample design.

Analysis reports important to this section

- Report B-9: "Accuracy and Coverage Evaluation Survey: Dual System Estimation Results," by Peter P. Davis

- Report B-11: "Accuracy and Coverage Evaluation Survey: Variance Estimates by Size of Geographic Area," by Michael D. Starsinic, Charles D. Sissel, and Mark E. Asiala

  Discussion

The dual system estimate shows that the Census 2000 undercount rate for the national household population is 1.18 percent, with a standard error of 0.13 percent. The net undercount for the 1990 census was estimated at 1.61 percent, with a standard error of 0.20 percent (see table 2, above). Comparisons by poststrata between 1990 and 2000 are necessarily inexact as the universe differs (2000 includes only the household population) and the exact poststrata definitions are different. Still, some comparisons are instructive. The standard error for owners was reduced from 0.21 percent to 0.14 percent, and the standard error for non-owners fell from 0.43 percent to 0.26 percent. The measured standard error fell for all comparable race/origin groups and for each age/sex group. The estimated standard error was comparatively high for the two groups estimated separately for the first time: Hawaiian and Pacific Islanders (2.77 percent) and American Indians and Alaskan Natives living off reservation (1.33 percent). As we will see, these groups also had high levels of inconsistent reporting between the census and the A.C.E. The estimated standard error for American Indians living on reservations fell dramatically from 5.29 percent to 1.2 percent. The standard error for Asians was 0.64 percent. For Hispanics, it was 0.38 percent, and for non-Hispanic Blacks it was 0.35 percent.

Table 4 gives the estimated percent net undercount and standard errors for the 64 major poststratum groups. The standard errors for several groups are above 1 percent and for a few small groups are up to 4 percent. Because the populations of these groups are small, their high variances will have only limited impact on geographic variance.

Table 4a: Census 2000 A.C.E. 64 Post-Stratum Groups - Percent Net Undercount

<table>
<tr><th colspan="2" rowspan="2">Race/Hispanic Origin Domain Number*</th><th rowspan="2">Tenure</th><th rowspan="2">MSA/TEA</th><th colspan="4">High Return Rate</th><th colspan="4">Low Return Rate</th></tr>
<tr><th>NE</th><th>MW</th><th>S</th><th>W</th><th>NE</th><th>MW</th><th>S</th><th>W</th></tr>
<tr><td colspan="2" rowspan="8">Domain 7
(Non-Hispanic White or "Some other race")</td><td rowspan="4">Owner</td><td>Large MSA MO/MB</td><td>0.81</td><td>0.01</td><td>0.36</td><td>-0.38</td><td>-3.62</td><td>-2.61</td><td>2.19</td><td>1.14</td></tr>
<tr><td>Medium MSA MO/MB</td><td>0.30</td><td>-0.12</td><td>0.46</td><td>-0.28</td><td>-4.39</td><td>-0.33</td><td>0.66</td><td>1.81</td></tr>
<tr><td>Small MSA & Non-MSA MO/MB</td><td>-0.25</td><td>0.14</td><td>0.44</td><td>0.30</td><td>2.29</td><td>2.61</td><td>2.09</td><td>2.71</td></tr>
<tr><td>All Other TEAs</td><td>1.84</td><td>-1.11</td><td>1.34</td><td>0.85</td><td>0.56</td><td>-0.16</td><td>0.15</td><td>1.59</td></tr>
<tr><td rowspan="4">Non-Owner</td><td>Large MSA MO/MB</td><td colspan="4">1.82</td><td colspan="4">1.02</td></tr>
<tr><td>Medium MSA MO/MB</td><td colspan="4">0.61</td><td colspan="4">2.83</td></tr>
<tr><td>Small MSA & Non-MSA MO/MB</td><td colspan="4">2.45</td><td colspan="4">3.61</td></tr>
<tr><td>All Other TEAs</td><td colspan="4">1.64</td><td colspan="4">4.08</td></tr>
<tr><td colspan="2" rowspan="8">Domain 4
(Non-Hispanic Black)</td><td rowspan="4">Owner</td><td>Large MSA MO/MB</td><td colspan="4" rowspan="2">1.63</td><td colspan="4" rowspan="2">-1.31</td></tr>
<tr><td>Medium MSA MO/MB</td></tr>
<tr><td>Small MSA & Non-MSA MO/MB</td><td colspan="4" rowspan="2">0.07</td><td colspan="4" rowspan="2">0.46</td></tr>
<tr><td>All Other TEAs</td></tr>
<tr><td rowspan="4">Non-Owner</td><td>Large MSA MO/MB</td><td colspan="4" rowspan="2">4.18</td><td colspan="4" rowspan="2">3.42</td></tr>
<tr><td>Medium MSA MO/MB</td></tr>
<tr><td>Small MSA & Non-MSA MO/MB</td><td colspan="4" rowspan="2">2.64</td><td colspan="4" rowspan="2">0.12</td></tr>
<tr><td>All Other TEAs</td></tr>
<tr><td colspan="2" rowspan="8">Domain 3
(Hispanic)</td><td rowspan="4">Owner</td><td>Large MSA MO/MB</td><td colspan="4" rowspan="2">1.46</td><td colspan="4" rowspan="2">0.04</td></tr>
<tr><td>Medium MSA MO/MB</td></tr>
<tr><td>Small MSA & Non-MSA MO/MB</td><td colspan="4" rowspan="2">1.66</td><td colspan="4" rowspan="2">1.08</td></tr>
<tr><td>All Other TEAs</td></tr>
<tr><td rowspan="4">Non-Owner</td><td>Large MSA MO/MB</td><td colspan="4" rowspan="2">3.52</td><td colspan="4" rowspan="2">4.98</td></tr>
<tr><td>Medium MSA MO/MB</td></tr>
<tr><td>Small MSA & Non-MSA MO/MB</td><td colspan="4" rowspan="2">4.88</td><td colspan="4" rowspan="2">10.74</td></tr>
<tr><td>All Other TEAs</td></tr>
<tr><td colspan="2" rowspan="2">Domain 5
(Native Hawaiian or Pacific Islander)</td><td colspan="2">Owner</td><td colspan="8">2.71</td></tr>
<tr><td colspan="2">Non-Owner</td><td colspan="8">6.58</td></tr>
<tr><td colspan="2" rowspan="2">Domain 6
(Non-Hispanic Asian)</td><td colspan="2">Owner</td><td colspan="8">0.55</td></tr>
<tr><td colspan="2">Non-Owner</td><td colspan="8">1.58</td></tr>
<tr><td rowspan="4">American Indian or Alaska Native</td><td rowspan="2">Domain 1
(On Reservation)</td><td colspan="2">Owner</td><td colspan="8">5.04</td></tr>
<tr><td colspan="2">Non-Owner</td><td colspan="8">4.10</td></tr>
<tr><td rowspan="2">Domain 2
(Off Reservation)</td><td colspan="2">Owner</td><td colspan="8">1.60</td></tr>
<tr><td colspan="2">Non-Owner</td><td colspan="8">5.57</td></tr>
</table>

- For Census 2000, persons can self-identify with more than one race group. For post-stratification purposes, persons are included in a single Race/Hispanic Origin Domain. This classification does not change a person's actual response. Further, all official tabulations are based on actual responses to the census.
- A negative net undercount denotes a net overcount.

Table 4b: Census 2000 A.C.E. 64 Post-Stratum Groups - Standard Error of the Net Undercount

Race/Hispanic Origin Domain Number*		Tenure	MSA/TEA	High Return Rate				Low Return Rate			
				NE	MW	S	W	NE	MW	S	W
Domain 7 (Non-Hispanic White or "Some other race")		Owner	Large MSA MO/MB	0.43	0.36	0.87	0.45	1.05	1.43	1.54	2.09
			Medium MSA MO/MB	0.85	0.28	0.42	0.38	1.52	0.84	1.10	2.79
			Small MSA & Non-MSA MO/MB	1.33	0.40	0.43	0.57	3.60	2.12	1.08	1.49
			All Other TEAs	1.06	0.39	0.97	1.66	2.17	1.21	0.65	1.89
		Non-Owner	Large MSA MO/MB	0.63				1.01			
			Medium MSA MO/MB	0.71				1.24			
			Small MSA & Non-MSA MO/MB	0.51				1.24			
			All Other TEAs	0.94				1.67			
Domain 4 (Non-Hispanic Black)		Owner	Large MSA MO/MB Medium MSA MO/MB	0.56				1.24			
			Small MSA & Non-MSA MO/MB All Other TEAs	1.07				1.86			
		Non-Owner	Large MSA MO/MB Medium MSA MO/MB	0.66				1.05			
			Small MSA & Non-MSA MO/MB All Other TEAs	0.96				2.08			
Domain 3 (Hispanic)		Owner	Large MSA MO/MB Medium MSA MO/MB	0.52				1.26			
			Small MSA & Non-MSA MO/MB All Other TEAs	1.01				2.09			
		Non-Owner	Large MSA MO/MB Medium MSA MO/MB	0.67				1.12			
			Small MSA & Non-MSA MO/MB All Other TEAs	1.55				4.12			
Domain 5 (Native Hawaiian or Pacific Islander)		Owner		3.83							
		Non-Owner		4.07							
Domain 6 (Non-Hispanic Asian)		Owner		0.87							
		Non-Owner		0.98							
American Indian or Alaska Native	**Domain 1** (On Reservation)	Owner		1.45							
		Non-Owner		1.42							
	Domain 2 (Off Reservation)	Owner		1.95							
		Non-Owner		2.02							

* For Census 2000, persons can self-identify with more than one race group. For post-stratification purposes, persons are included in a single Race/Hispanic Origin Domain. This classification does not change a person's actual response. Further, all official tabulations are based on actual responses to the census

At the state level, the median coefficient of variation (CV) for state population totals dropped from 0.41 percent in 1990 to 0.24 percent in 2000. More important, the median CV for the congressional districts dropped from 0.5 percent to 0.3 percent. Similar drops in the CV of 40 percent to 50 percent were estimated for counties and places larger than 100,000.

This decrease in sampling variance is due to the much larger sample size of the A.C.E. relative to the PES: 300,913 housing units in 11,303 clusters for the A.C.E., versus 165,000 housing units in approximately 5,000 clusters for the 1990 PES. Better measures of population size in the sample selection of block clusters, better subsampling methods, better methods of treating "small blocks," and a reduction in the variability of sampling weights all contributed to this reduction.

One simple analysis was to compare estimated undercount rates from the A.C.E. with estimated confidence levels. We can compare the undercounts among the 64 post-strata groups (collapsed over age and sex) with their confidence intervals. Of course, care must be taken in this analysis, with proper correction for multiple comparisons.

This analysis clearly showed that the A.C.E. results cannot be dismissed as "simply variance." (See Report B-9, "Dual System Estimation Results") A clear pattern of minority undercount and a most pronounced undercount of minority renters emerged. This pattern is consistent with differential undercount patterns found in all prior censuses.

Consistent Reporting of Census Day Residence

Conclusions for this Section

The consistency of reporting of Census Day address should be better than in 1990 due to the interviews occurring closer to Census Day and better quality interviewing made possible with the CAPI instrument.

Analysis Reports Important to this Section

- Report B-5: "Accuracy and Coverage Evaluation Survey: Person Interviewing Results," by Rosemary L Bryne, Lynn Imel, and Phawn Stallone

Discussion

Proper application of the DSE model requires consistent reporting of Census Day residence between the P and E-samples. If a person who was sampled in the P-sample reports a different Census Day residence than he/she reported in the E-sample, then that person could be considered both missed (based on the P-sample) and correctly enumerated (based on the E-sample), or conversely, both enumerated (based on the P-sample) and not correctly enumerated (based on the E-sample). Since many people fall only into either the P or the E-sample, measuring consistent reporting is an important task. When a person is in both the P and the E-sample, consistent reporting between the two systems is not a problem because we use the same interview for both samples. However, some individuals have two interviews, one in the P-sample and one in the E-sample. For example, we use the initial A.C.E. interview for individuals in the P-sample to determine their correct Census Day residence. However, if an individual was missed by the A.C.E. but included in the initial census, we would use the A.C.E. follow-up interview to determine Census Day residence. Even for matched people, if the person was duplicated by the census, we might have a different interview at each identified census household. Since these interviews use different survey questionnaires, and are administered at different times by different interviewers to potentially different respondents, there is a chance that the two interviews could result in different correct Census Day residences for the same person. Inconsistency in Census Day address reporting can influence the dual system estimates.

The 1990 Evaluations (P studies) measured the consistency of reporting Census Day addresses in the PES by comparing the reinterview to the production results. (See P - 4, "Address Misreporting"). One problem in 1990 was the misreporting of Census Day addresses, with an estimated 0.7 percent of the P-Sample being erroneously reported as nonmovers. (See P - 4, "Address Misreporting") The 2000 A.C.E. improves on 1990 PES, in particular because the use of the CAPI instrument requires the interviewer to ask all questions in the interview form, a vast improvement over the 1990 PES pencil and paper interview.

There are two factors that should have increased the consistency of reporting census day addresses. First is the time schedule. The A.C.E. interviews were conducted much closer to Census Day than were the 1990 PES interviews. This would normally increase the accuracy of recall. In addition, the CAPI interview instrument forced the interviewers to ask all probes as to Census Day residence, again probably increasing consistency. In addition, the A.C.E. interview usually used proxy respondents for movers where the 1990 PES normally interviewed the mover household themselves. This has an unknown effect on consistency; however, we have no direct data on this at this time.

Matching error

Conclusions for this Section

The matching error rate for 2000 is low with indications that it is substantially lower than that achieved in 1990.

Analysis Reports Important to this Section

- Report B-6: "Person Matching and Follow-up Results, by Danny R. Childers, Rosemary L. Byrne, Tamara S. Adams, and Roxanne Feldpausch"

Discussion

Matching error refers to assigning the incorrect code to a P-sample record. Matching error can consist of assigning a code of "matched" to a true nonmatch case and vice-versa. It can also consist of assigning an unresolved code to a case that has sufficient information. Matching errors can directly influence the final dual system estimates. Matching errors have both a random and a systematic component. The random component will be partially reflected in the overall variance estimates.

Matching error was measured in 1990 by conducting a rematch study, that is, by going back after the fact and rematching a sample of cases. (P-7, "Estimates of P-sample Matching Error from a Rematching Evaluation," P-10, "Measurement of the Census Erroneous Enumeration Clerical Error Made in the Assignment of Enumeration Status"). A study of clerical error in the 1990 PES found error in coding matches (P-5a, "Analysis of Fabrications from Evaluation Follow-up Data") and erroneous enumerations (P-6, "Fabrication in the P-sample -- Interviewer Effect"). In 1990, codes were entered into a computer system, but the actual matching and duplicate searches were done using paper. We expected A.C.E. matching to be better controlled and more efficient because the clerical matching and quality assurance were fully automated and the matching was conducted at a single site. The automated interactive system does not prevent all matching error but should reduce the chances for error significantly. Our results confirmed these expectations.

The 1990 matching system matched both nonmovers (within E-sample area) and in-movers (who could be coded and matched in any area). The 1990 mover match system not only included several additional steps (mainly to geographically code the Census Day address) but was also completely clerical. For the A.C.E., all matching was within the sample area or its surrounding blocks. The 2000 nonmover matching system was largely automated. The system was used to match both nonmovers and out-movers. The system was significantly more automated, with less clerical matching, and all clerical matching operations were conducted at one location. Comparisons to 1990 must take these changes into account.[6]

Other examples of the improvements in matching included:

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[6]The A.C.E. design treats movers differently than in 1990, using a procedure called PES-C, rather than the 1990 procedure, PES-B. In 1990, movers were sampled where they lived at the time of the PES interview. The Census Bureau then searched the census records at the movers' April 1 usual residence to determine if they had been correctly enumerated in the census. This procedure was PES-B. In the PES-C procedure, the Census Bureau combined information on movers from two sources to produce an estimate of movers who were missed in Census 2000. First, an estimate of the total number of movers was calculated based on people who moved into the A.C.E. sample blocks between April 1, 2000, and the time of the A.C.E. interview. Second, the rate at which movers matched to Census 2000 was calculated by matching the Census Day residents of the A.C.E. sample housing units to the initial census records.

- Electronic filtering allowed searching within a particular search area based on first name, last name, characteristics, and addresses. For example, the system allowed searching for all people named George, all people whose last name began with an H, all people on Elm Street, or all people between 30 and 40 years old.

- Only particular codes that fit the situation were allowed. For example, only P-sample nonmatch codes could be assigned a P-sample nonmatch after follow-up code.

- The electronic searches for duplicates reduced the tedious searching through paper lists of census people. The searching in 1990 was limited to printouts in two sorts: last name and household by address. In 2000, the clerks could search electronically by name, address, and other characteristics to help identify duplicates.

- Computer images of the Census questionnaire were easily accessible.

- The system monitored whether the matcher completed all the necessary searches such as looking for duplicates.

- Built in edits checked for consistent coding. For example, codes that applied to a household were assigned to all people in the household, such as a geographic code.

- The system automatically assigned certain codes, minimizing coding error.

- A code to indicate that the case needed review at the next level of matching was available to the clerical matchers. This code allowed them to flag unusual cases to be done by a person with more experience.

- All quality assurance for the clerical matching was automated. Therefore, the quality assurance component of the operation could not be skipped in 2000.

- Clerical matching was centralized at the National Processing Center instead of having different groups of matchers in seven processing offices, as was done in 1990. Forty-six technicians were hired in September 1999 and thoroughly trained in the design of the A.C.E. and matching of people and housing units. These technicians performed the quality assurance for the clerical matchers. Additionally, 16 analysts were our most experienced matchers. The analysts performed the quality assurance for the technicians and handled the most difficult cases.

The results of the matching quality assurance program constitute the primary information available for assessing the matching operation. This program gives us information about the level of error relative to that of our most experienced matching specialists. It should be noted that many of these same individuals participated in the 1990 PES. The results of the quality assurance process noted above and in B-6 show that we achieved a very high level of matching quality. The majority of cases were computer matched. The change rate for the clerical operation (the rate of cases that the next level of review concludes must be changed) is very low and is any event an upper bound on the error rate.

A.C.E. Fabrications

Conclusions for this Section

Fabrication was more tightly controlled in the A.C.E. than it was in the 1990 PES because of the tighter field management control made possible by the CAPI instrument.

Analysis Reports Important to this Section

- Report B-6: "Accuracy and Coverage Evaluation Survey: Person Matching and Follow-up Results," by Danny R. Childers, Rosemary L. Byrne, Tamara S. Adams, and Roxanne Feldpausch

Discussion

Inclusion of fictitious people in the dual system estimates can create a bias unless the number of fictitious people is controlled for a small level. Fictitious records have little chance of being matched between the P and E-samples, which means that they can erroneously increase the undercount estimates. Fictitious records, of course, should not be included in either the P-sample or the census. Fabrications in the initial census are measured by the E-sample (See below). Here we concentrate upon fabrications in the P-Sample.

In 1990, the level of fabrication in the P-sample was measured by three studies evaluating different measures of potential fabrication. The first study (P-5, "Analysis of P-sample Fabrications from PES Quality Control Data") evaluated interviewer fabrication detected in the quality control operation (and rectified by the QC operation), as well as fabrication detected in the follow-up operation. The estimated number of fabricated persons remaining, at the national level, after the quality control operation was approximately 0.13 percent. The second study, using data from the 1990 Evaluation Follow-up, concluded that an additional 0.09 percent (weighted to the PES unweighted totals this figure represents 0.03 percent of the total sample) of the P-sample follow-up interviews included in the evaluation sample should have been coded as fictitious. (P-5a, "Analysis of Fabrications from Evaluation Follow-up Data"). This evaluation was designed to identify P-sample fabrication not detected by the quality control procedure. A third study, (Project P6: "Fabrication in the P-sample -- Interviewer Effect") compared the nonmatch rates of interviewers working in similar areas, while assuming that deviations from the nonmatch rate may have indicated undetected curbstoning. This study used a model to predict nonmatch rates and showed that between 0.9 percent and 6.5 percent overall of the interviewers were found to have high nonmatch rates, high rates that may have corresponded to dishonesty in their data collection.

We have evaluated potential fictitious records in the A.C.E. by reviewing detailed quality assurance results that document the level of detected fabrications in the initial A.C.E. interview, as well as measures of residual fabrication. In addition we have the results of the Person Follow-up interviewing, which should have detected whole household P-sample fabrications not detected by the interviewing quality assurance program. These sources allowed us to evaluate the level of A.C.E. fabrication.

The evidence indicates that the quality assurance was successful in controlling A.C.E. fabrications. Because the A.C.E. interview was taken on the CAPI instrument, it was "time stamped" so that field staff could use automated reports to quickly detect interviewers who reported odd interviews, such as rapid multiple interviews, interviews at odd hours (such as late night interviews), and other similarly unbelievable interview results. The CAPI instrument allowed field management staff to tightly monitor the behavior of the A.C.E. interviewers.

In addition, we examined the data to look for information relating to clusters, because fabrication is often highly clustered. An otherwise acceptable interviewer might, for example, suddenly fabricate his or her last assignment. The matching analysts kept a detailed record of any unusual clusters. These analysts could request special questions during follow-up or send additional cases to follow-up interviewing if they questioned the integrity of one interviewer's results. These records would provide an additional clue to whether there was substantial, clustered fabrication in the P-Sample. Analysts had the discretion to remove cases they believed to have been fabricated.

Missing data

Conclusions for this Section

The level and pattern of missing data in the A.C.E. is comparable to that of the 1990 PES. The effect of the missing data on the overall A.C.E. quality is similar to that experienced by the 1990 PES and documented in the P studies.

Analysis Report Important to this Section

- Report B-7: "Accuracy and Coverage Evaluation Survey: Missing Data Results," by Patrick J. Cantwell

Discussion

Missing data can introduce uncertainty into DSE results. Missing data can contribute to variance and, if the missing data models are poorly specified, can also contribute to bias and differential bias.

Missing data has three components:

- Whole household noninterviews
- Unresolved match, residence, or enumeration status
- Missing demographic characteristics

This section focuses on the first two components of missing data: whole household noninterviews and unresolved match, residence, or enumeration status. The third component of missing data, missing postratification variables, will generally result in correlation bias or synthetic error and will be evaluated in connection with the analysis reports on those topics. Missing post-stratification variables tend to lead to correlation bias or synthetic error because this omission can increase heterogeneity and inconsistent post-stratification between the initial census and the A.C.E. High levels of missing data, particularly for unresolved match, residence, or enumeration status, also tend to increase variance. We have not evaluated how this type of missing data by itself increases variance because this component is largely picked up in our measure of sampling variance.

The 1990 PES dealt with movers by using Procedure-B. Under Procedure-B, missing data can occur when the interviewer fails to get information from the respondent, in either the initial interview or the follow-up interview, or the missing data can occur during follow-up. The 1990 PES had low rates of initial missing data, but a greater number of unresolved cases in the follow-up process. Procedure-B required geocoding the matching, making it possible that completed "mover" cases could not be used because of ambiguities in the geographic coding. Procedure- B, therefore, resulted in initially low rates of missing data but was responsible for additional missing data in later processes.

The effects of missing data on the 1990 results were studied in two ways. First, the modeled results were compared to the results of further field work on the nonresponse cases (P-3, "Evaluation of Imputation Methodology for Unresolved Match Status Cases"). The field work largely validated the models. This alone is extremely important work as it clearly demonstrated that some of the extreme missing data adjustments sometimes proposed (for example assuming all nonresponse cases were missed) were not supported by the data. Second, additional 1990 studies (P-1, "Analysis of Reasonable Imputation Alternatives") tended to show the robustness of the results to reasonable alternatives.

There have been two important changes for Census 2000 that might affect missing data rates. First, we expected that the level of missing data in the A.C.E. interview might be higher because of a change in how we treated movers. In 1990 the Census Bureau only needed to interview the current residents, whereas in Census 2000, interviewers required information about both the current (A.C.E. Interview Day) residents and the Census Day residents. On the other hand, Procedure C, which we used in the A.C.E., eliminated the need to geographically code the Census Day address of "in-movers," thus eliminating one potential source of missing data. Second, the CAPI instrument kept the interviewer on the correct set of questions and allowed for tight managerial control.

The A.C.E. used a different missing data model for unresolved match and residence status. The 1990 model was based on hierarchical logistic regression, while the 2000 model used the far simpler "Imputation Cell Estimator." The input data and behavioral assumptions between the two models are similar but not identical.

The A.C.E. was able to maintain high quality interviewing and keep the level of missing data to low levels. This low level of missing data minimizes the effect on the final estimates of the missing data assumptions.

Noninterview in the P-sample: A.C.E. interview rates were very high. Among occupied housing units, the rates were 97.1 percent for Census Day and 98.8 percent for A.C.E. Interview Day. This compares to 98.4 percent (unweighted) in the 1990 PES. Due to the high response, most of the changes due to the noninterview adjustment factors applied were very small. This result helps to keep down the variance of the survey weights.

Unresolved resident status in the P-sample: The proportion of people with unresolved residence was very low, 2.2 percent. Thus, it appears that missing this item has only a minor effect on the estimation process. The missing data procedures assigned an average resident probability of 82.6 percent to people with unresolved resident status, which was, as designed, lower than the average rate among people with resolved status (98.2 percent).

Unresolved match status in the P-sample: Only 1.2 percent of the sample had unresolved match status, compared to 1.8 percent in the 1990 PES. We assigned an average match rate of 84.3 percent to people with unresolved match status, compared to 91.7 percent for those with resolved status. The low rate of unresolved match status implies only a small effect on the estimation.

Unresolved enumeration status in the E-sample: About 2.6 percent of the E-sample had unresolved enumeration status; it was 2.3 percent in the 1990 PES. The average rate of correct enumeration for people with unresolved status was 76.2 percent as compared with the 95.9 percent for those with resolved status.

The level and direction of the differences between resolved and unresolved cases are generally what we expected and are explainable by the design of the missing data estimation.

Balancing error

Conclusions for this Section

Although detailed information is not yet available, the evidence now available does not permit us to conclude that there was no balancing error in 2000. One concern is that a number of E-sample cases were coded as correct even though they were outside the search area. This concern is discussed in a following section.

Analysis Report Important to this Section

- Report B-8: "Accuracy and Coverage Evaluation Survey: Decomposition of Dual System Estimate Components," by Thomas Mule

- Report B-18: "Accuracy and Coverage Evaluation Survey: Effect of Targeted Extended Search," by Douglas B. Olson

Discussion

Balancing error occurs when the set of correct enumerations records defined and measured in the E-sample does not correspond to the set of records against which P-sample matching is allowed. An important type of balancing error occurs when the search area, as defined and implemented in the E-sample, does not correspond to the search area as defined and implemented in the P-sample. The dual system model first determines the number of individuals who are correctly in the initial census (through the E-sample) and then the proportion of the true population that is correctly in the census (through the P-sample). If the E-sample and the P-sample use different definitions of "correctly in the census," the model will not work. Specifically, if the P-sample allows matches, (that is, treats a person as correctly enumerated), if he/she was found anywhere in a wide area, but the E-sample treated as erroneous (that is, not correctly enumerated) any census record not in its correct block, then the P and E-samples are using different definitions about what constitutes a correct enumeration. Obviously, there would also be balancing error if the E-sample definition was broad, but the P-sample definition was narrow.

Balancing error, especially geographic balancing error, was a major concern in the 1980 post enumeration survey. The E-sample in 1980 counted a person as being correctly in the census only if he or she was counted in the correct Enumeration District. Enumerations outside the correct enumeration district were considered erroneous. However, the P-sample in 1980 searched several enumeration districts looking for a match. Thus some P-sample people were considered correctly enumerated because they matched to census records that would have been considered erroneous had these records been included in the E-sample. This particular problem was addressed in the 1990 PES by using identical search areas for nonmovers. A concern remained for movers. (P-11, "Balancing Error Evaluation").

The A.C.E. used a somewhat more complex balancing design than did the 1990 PES. One minor change was that the search area in 2000 was somewhat smaller, encompassing only the first ring of blocks of housing units around a census block. More important, not all cases were eligible for searching, coding, and matching in the surrounding ring; only whole household nonmatches and E-sample geocoding errors were eligible for surrounding block search. This search area is referred to as "Targeted Extended Search" or TES. The TES surrounding block search was also performed on a sample basis.

A major goal of extended search, whether targeted or not, is to reduce the variance of the estimators, especially for small estimation cells where census geocoding errors will not tend to cancel out. To assess the effect of TES, we compared correct enumeration rates and match rates for TES and non-TES cases.

Extended search can reduce A.C.E. bias due to A.C.E. P-sample and E-sample geocoding errors. If an A.C.E. address listing includes housing units outside the actual block, as defined by the census, an attempt to match only to the sample block will usually result in nonmatches for all units actually outside the block. This situation can lead to a high false measure of census omission and extending the search to the surrounding blocks reduces this bias. Extended search essentially converts a first order matching bias to a second or third order sampling bias.

In addition, it is possible for the A.C.E. E-sample follow-up to incorrectly code a housing unit as inside a block when the unit is actually just outside the block. Without extended search, this discrepancy would result in a unit coded "correctly enumerated" that was actually a geocoding error. With extended search, the enumeration of the unit is correct whether coded to the actual block or a surrounding block. Obviously, if the unit was actually located completely outside the search area, coding it to the block or a surrounding block (that is, "correctly enumerated") would be an error. There is evidence that this type of coding sometimes occurred, as discussed below.

A review of the results of Targeted Extended Search program (TES) has indicated an imbalance between P-sample matches to the surrounding block and E-sample enumerations coded as "correctly counted in the surrounding block." Ideally, these should be similar. This result raised concerns. However, it is consistent with the presence of a small amount of A.C.E. P and E-sample geocoding error. Similar results were encountered in 1990. An imbalance may be due to the geographic miscoding of E-sample cases discussed below.

Errors in Measuring Census Erroneous Enumerations

Conclusions for this Section

In general, the evidence suggests that with the possible exception of geographic mis-geocoding, E-sample coding errors were controlled at least as well as in 1990. However, preliminary results from an early A.C.E. evaluation indicate that a number of E-sample cases coded as correct enumerations were in fact outside of the search area. That means that they should have been coded as Erroneous Enumerations and subtracted from the DSEs. This error could introduce an upward bias in the DSE.

Analysis Report Important to this Section

- Report B-6: "Accuracy and Coverage Evaluation Survey: Person Matching and Follow-up Results," by Danny R. Childers, Rosemary L. Byrne, Tamara S. Adams, and Roxanne Feldpausch

In addition,

- DSSD Memorandum Series T-6: "Additional Geographic Coding for Erroneously Enumerated Housing Units," by Danny R. Childers and Xijian Liu.

Discussion

Erroneous enumerations occur in the initial census in the following circumstances:

- when an individual had another residence where he or she should have been counted on Census Day
- when an entry is fictitious
- when entries are duplicated
- when an individual lived in a housing unit subject to geocoding error
- when the Census Bureau had insufficient information for matching and follow-up

Errors in measuring census erroneous enumerations can have a serious and direct impact on the A.C.E. For example, a systematic tendency in A.C.E. processing to code census fictitious cases ("curbstoned cases") as E-sample follow-up "noninterviews" leads to an incorrect estimate of the number of respondents correctly enumerated in the initial census. A tendency to "give the census the doubt" can result in people who move out before Census Day coded as correct enumerations. While the overlapping of the P and E-samples will lend considerable robustness to the A.C.E. estimates, both systematic and random errors can be expected to occur.

E-Sample cases are either coded during the initial matching operation or coded based on information gathered during A.C.E. follow-up. For the A.C.E., we assessed errors in measuring census enumeration by analyzing the matching systems' quality assurance results, as well as by using information from A.C.E. follow-up. The quality assurance program should have indicated any clerical problems in assigning enumeration status.

The Census Bureau found clerical error in assigning erroneous enumerations in 1990 (P-10, "Measurement of the Census Erroneous Enumeration Clerical Error Made in the Assignment of Enumeration Status"). The improvements in Census 2000 clerical matching (described earlier) should have improved the assignment of erroneous enumerations. The identification of duplicates was closely monitored to assure that the duplicate search was done within the block cluster and in the surrounding blocks for TES clusters. The follow-up interview has been improved to instruct the interviewer to conduct sufficient searches for people to allow accurate coding of fictitious people. The conclusion was that the follow-up interviewing was in both managerial and statistical control.

The A.C.E. matching and follow-up quality assurance results referenced in the *Matching and Follow-up* section above indicate that these processes were well controlled and that these errors were no worse than in 1990.

The one area of concern is the level of correct coding of E-sample cases that were actually outside the search area. Preliminary results from an early A.C.E. evaluation indicate that a number of cases that were coded as "correctly enumerated" were in fact outside the search area. This means that the E-sample process accepted a number of records, as correct when they were in fact erroneous. This would understate the gross census overcoverage rate and thus overstate the census net undercount.

Correlation bias

Conclusions for this Section

Correlation bias is documented for the Black male population and is almost certainly present for certain non-Black populations, including the non-Black Hispanic population. Unfortunately, evidence on the level of correlation bias is weak.

Analysis Reports Important to this Section

- Document 12: "Accuracy and Coverage Evaluation Survey: Correlation Bias" by William R. Bell
- Document 4: "Accuracy and Coverage Evaluation Survey: Demographic Analysis Results" by J. Gregory Robinson

Discussion

Correlation bias is the term frequently used to refer to error caused by individuals systematically missed in both the initial census and the coverage measurement survey. In its purest form, dual system estimation assumes that the chance of being included in the P-sample is independent of the chance of being correctly included in the initial census. Although this assumption has proven useful in providing a better estimate of the population, it is, of course, unlikely to be absolutely true. Correlation bias can occur from two sources. First, it can be caused by inherent heterogeneity within the post-strata. It can also arise when the event of being enumerated in the census changes the probability of being included in the A.C.E.

Even within post-strata there may be unobservable sub-groups with differing chances of being included in each system. There is also quite likely some group (of an indeterminate size) whose probability of being included in any survey is so low as to be effectively zero. Correlation bias will tend, therefore, to lead to an underestimate of the population. Dual system estimation will estimate some, but not all, of the people omitted from the initial census.

Correlation bias is a bias in the dual system estimator. That is, it must be considered in light of both the initial census interview and the A.C.E. interviewing and processing. Correlation bias due to heterogeneity can be reduced either because the initial census was more successful in including the "hard to count,"or because the A.C.E. was more successful in including the "hard to count." The census paid advertising and outreach campaign, especially that targeted to ethnic minorities including Hispanics, could have the effect of reducing correlation bias in the 2000 DSE.

To measure correlation bias, one would ideally like to have an external measure of "truth." Demographic analysis, especially demographic sex ratios, have in the past provided an external measure that, while not perfect, is useful because it is not subject to many of the limitations of the initial census or the dual system estimates. As discussed later in this document, comparisons with demographic analysis are increasingly difficult.

Using demographic results, the 1990 studies detected a clear pattern of correlation bias in the 1990 PES (P-13, "Use of Alternative Dual System Estimators to Measure Correlation Bias"). Correlation bias was especially strong for adult Black males, a group that dual system estimation methodology seems to underestimate.

Recent criticisms of the 1990 studies seem to point to the fact that these studies underestimated the level of correlation bias in the 1990 PES. This conclusion follows from the fact that, in general, correlation bias tends to lower the estimated population, while other measurement errors tend to raise the estimate. Correlation bias and the other kinds of errors therefore may have tended to cancel each other out. However, this reasoning applies to comparisons of the 1990 PES estimates to demographic analysis population totals. If comparisons are instead made to the demographic analysis sex ratios (as was done in the P-13 report), and if the other measurement errors are not very different between males and females, then these other measurement errors should tend to cancel out and have little effect on resulting estimates of correlation bias. Note that comparability problems arising from Black Hispanics, whom DA assigns to Black and A.C.E. assigns to non-Blacks, are expected to have minor effects on sex ratios for 2000. However, we have not fully analyzed the data that supports this expectation.

An additional problem is that since demographic analysis provides national results, one must model how these errors might distribute themselves by post-strata. Several alternative models have been tried. (P-13, "Use of Alternative Dual System Estimators to Measure Correlation Bias"; Bell, "Using Information from demographic analysis in Post-Enumeration Survey Estimation," 1993).

A final problem arises from the nature of the preliminary 2000 demographic analysis results, discussed below. These results imply a level and pattern of net undercount different from that in any previous census studied or from that measured by the A.C.E.. Indeed, even some of the comparisons of sex ratios, normally the most robust aspect of demographic analysis, are quite different from those observed in previous censuses. These results make quantifying correlation bias even more difficult for Census 2000 than in previous censuses.

The level of correlation bias in the A.C.E. might be larger than that in the 1990 PES because of the use of Procedure C for movers. Procedure C was designed to reduce matching error by eliminating mover matching. However, since this procedure calls for the reconstruction of the Census Day household, its use may increase correlation bias because it may result in the "missing" of individuals only tenuously connected to the household. Weighting the out-mover match-rate by the number of in-movers may partially, but probably not completely, compensate for the possible increase in correlation bias. Even among out-movers, those more likely to be enumerated in the initial census may be more likely to be picked up in the A.C.E. interview. Because of this potential correlation, we might overestimate the mover match rate.

Our analysis of correlation bias in the 2000 estimates was, as in 1990, based on the sex ratios from demographic analysis. It is limited to only measuring the correlation bias of Black adult males and non-Black adult males. The method assumes no correlation bias for females and cannot be applied to the A.C.E. estimates for children. Essentially, it assumes that any shortfall of the number of males relative to females, as implied by the demographic analysis sex ratios, is attributable to a correlation bias for males. This analysis demonstrates the presence of correlation bias for adult Black males. The implied level is similar to that observed in 1990. Specifically, our analysis concludes that there is significant correlation bias for adult Black males 18-29 and 30-49 at levels very similar to 1990. There also is significant correlation bias for adult Black males 50+ that is smaller in magnitude than in 1990. Comparisons to demographic analysis sex ratios suggest at most small amounts of correlation bias for non-Black males 30-49 and 50+. The correlation bias estimates for these groups are very small, though they were not much larger in 1990. Due to inconsistency of demographic analysis and A.C.E. data for non-Blacks 18-29, we cannot estimate correlation bias for males in this group.

Determining the level of correlation bias for the non-Black population is problematic because for some age groups, demographic analysis sex ratios imply fewer males than measured by the A.C.E. Taken at face value, this result would mean either negative correlation bias for males (which has never been observed and is difficult to explain) or larger correlation bias for females than for males. Positive correlation bias for females is not only possible but likely. However, what is also likely is that using initial DA to measure correlation bias for non-Blacks using sex ratios has become problematic. This conclusion is important since the majority of the Hispanics, as well as of course other minority groups, are non-Black. A frequently expressed concern about the DSE methodology is the possibly large level of correlation bias for Hispanics.

This analysis only detects differential correlation for the Black and non-Black population. We have no measure for correlation bias for children or females, nor any separate measure for Hispanics, Asians, or other separate "non-Black" groups.

We also examined records and reports for any indication of correlation bias due to causal dependence, that is, any indication that participation (or non-participation) in the initial census directly influenced participation in the A.C.E.. For example, we looked at the number of letters (approximately 80) received from households that were reluctant to participate in the A.C.E. because they had already sent in their census form. We looked for reports from the regional offices to see if there was any indication of improper contact between the census enumerators and the A.C.E. interviewers. We found no reports or other evidence to support a problem with causal dependence.

There were also concerns about the effects on correlation bias of the "late census adds" and the higher level of imputations. This is discussed below. (See Other Measurement and Technical Errors.)

Synthetic Assumptions

Conclusions for this Section

Local census heterogeneity exists and affects the quality of both the adjusted and unadjusted census results. Properly accounting for the synthetic bias in the basic functions could potentially reverse a finding of small improvement, or small deterioration, from adjustment. This effect warrants further examination.

Analysis Report Important to this Section

- Report B-14: "Accuracy and Coverage Evaluation 2000: Assessment of Synthetic Assumptions" by Donald J. Malec and Richard A. Griffin

Discussion

Synthetic estimation error differs from the other measurement errors discussed in this document because it is not directly related to the accuracy of the dual system estimates themselves, but rather to the distribution of the measured net undercount to local areas and demographic subgroups.

Another important difference between synthetic error and other types of A.C.E. error is that local heterogeneity is present in the unadjusted census; this local heterogeneity will affect the quality of census results even before A.C.E. adjustment. While this local heterogeneity is not, strictly speaking, synthetic error, since no synthetic estimation is involved, the effect of local heterogeneity on the accuracy of the population estimates is similar. If local heterogeneity in the initial census is correlated with post-stratification variables, then the DSE/synthetic estimation process can reduce this heterogeneity. However, if a crew leader applied the census procedures in a way that resulted in a locally higher net undercount, then the DSE/synthetic model would not correct for this effect locally. Evaluations of the synthetic assumption help us to understand residual heterogeneity in both the initial and the corrected census.

Evaluations of the synthetic assumption are necessarily indirect. Because the A.C.E. is based on a sample, it may be inefficient at detecting truly local heterogeneity. Attempts at measuring local heterogeneity at the block cluster level suffer from the problem that the A.C.E. is not designed to directly measure the undercount, even for the sample clusters. Targeted extended search and large-block subsampling, for example, both allow matching beyond the sample segments. The A.C.E. is designed to measure undercount at high levels, not at the local level.

However, other data are available for all census areas. Some of these data may be related to the net undercount, although in perhaps complex ways. These data include the level of census whole person imputations and the level of census ID's removed from the census as part of HUDO and then reinstated. These can be tabulated at different levels than the A.C.E. poststrata. For example they can be tabulated for census region crossed by the other A.C.E. post-stratification variables (Attached). These analyses show that individual census procedures had different impacts in different census regions, even controlling for A.C.E. post-stratification variables. What one does not know, of course, is whether these procedures corrected for an underlying differential in coverage or created a new level of geographic differential in coverage.

The analysis of the data indicates variation within the poststrata for variables that might be related to the net undercoverage. If indeed, these indications are correct, the undercount in the unadjusted census varies not only between poststrata but also within poststrata. The A.C.E. adjustment process will not remove any differential patterns of undercount within poststrata. They will still be present within the data. Since this uneven census coverage is present in both the adjusted and unadjusted results, it does not seem to greatly affect the <u>relative</u> accuracy of the two sets of population estimates.

A productive approach is to use "artificial population" analysis. This analysis looks at census operational measures available for all areas, scales them to be the size of the gross undercount or overcount, and then analyzes the results to assess the impact of local geographic heterogeneity on census and A.C.E. accuracy. This analysis looked at several variables, these included:

Surrogates for Gross Omissions:

- Number of non-GQ[7] persons less the number of persons in whole household substitutions.
- Number of non-GQ persons with two or more item allocations.
- Number of non-GQ persons whose household did not return a questionnaire by mail, etc.

Surrogates for Gross Erroneous Enumerations:

- Number of non-GQ persons less those for whom the date of birth was allocated consistent with age.
- Number of non-GQ persons less the number of whole household substitutions.
- Number of non-GQ persons whose household did not return a questionnaire by mail, etc.

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[7]GQ means Group Quarters, that is, prisons, long-term care facilities, college dorms, and other group living arrangements.

These surrogates were chosen because they roughly correlated with the number of the A.C.E. nonmatches and A.C.E. E-Sample erroneous and incomplete enumerations for the sampled block clusters. Of course, for the artificial population analyses, we looked at both sample and nonsample blocks.

Assessments of the 1990 PES were concerned with the accuracy of the synthetic assumption for low levels of geography, such as blocks. Our assessment of the synthetic assumption in the A.C.E. accepts that perfect homogeneity cannot exist at the block level. The Census Bureau's evaluation of synthetic error, therefore, focuses on whether heterogeneity at the local level is so great as to prevent an improvement from using the A.C.E., not on whether the post-strata are absolutely homogeneous.

The analysis of the relative effect of synthetic error indicated that for state level count estimates (numeric state accuracy) three out of four loss functions are probably underestimating the true gains from adjustment (Report B-14, "Assessment of Synthetic Assumptions"). Thus, correcting for the bias would not change the loss function results.

For state shares, the analysis indicated a small effect of synthetic error on the loss function. Thus, for cases where the census loss and the A.C.E. loss were quite close showing a small improvement for adjustment, correcting for synthetic error could reverse the direction indicating a small decrease in accuracy by adjusting. This result warrants further investigation.

For congressional district share estimates, the evidence is mixed. That is, some analyses indicated that ignoring synthetic error in the loss function would overstate A.C.E. accuracy. Other analyses indicated that this would overstate census accuracy. That is, some analyses indicated that the loss function measures would be conservative. For other analyses, the results were that the effect could be large enough that they could reverse a favorable finding for adjustment. These analyses would indicate that for congressional districts, loss function results that indicate a small or even moderate improvement from adjustment could be misleading. Correctly accounting for synthetic error would reverse the finding implying greater census accuracy in these cases.

Other Measurement and Technical Errors

Conclusions for this Section

Available evidence does not indicate any appreciable increase in the level of any of these other measurement and technical errors over what was experienced in 1990.

Analysis Reports Important to this Section

- Document 10: "Accuracy and Coverage Evaluation Survey: Consistency of Post-Stratification Variables," by James Farber

Discussion:

The coverage measurement process is subject to several other kinds of measurement errors that need to be noted, including technical ratio bias, contamination error, and inconsistent post-stratification.

Technical ratio bias is well documented in the statistical literature and occurs when the expectation (statistical average) of a ratio differs from the expectation of the numerator divided by the expectation of the denominator. Technical ratio bias in survey estimates is usually not important unless the sample size is small. Usually, a sample size of thirty independent observations is adequate (Cochran, 1963). The dual system estimator is a ratio estimator and as such is subject to ratio bias. Further, since the Procedure C treatment of movers is also a ratio estimator, that may introduce a further ratio bias. The A.C.E. is designed to guard against large ratio bias by requiring a minimum cell size for both the post-stratum and the number of out-movers in the Procedure C estimate. While we did not expect technical ratio bias to be a problem in the A.C.E.

Technical ratio bias was shown to be small, as expected. (B-2, "Quality Indicators of Census 2000 and the Accuracy and Coverage Evaluation")

Contamination error occurs when the conduct of the coverage measurement survey affects how people react to the initial census in the sample areas. If contamination occurs, the coverage measurement survey may no longer reflect the error for the population as a whole, even if it correctly measures the coverage ratios for the sample areas. Contamination error has affected past coverage measurement surveys. The 1980 coverage measurement study (the PEP) was based on the April Current Population Survey, which had been conducted between Census Day and the start of NRFU. Evidence pointed to contamination error (See Fay et al., 1988). Prior to the 1998 Dress Rehearsal, contamination error was a major concern. See, for example, Griffiths, "Results from the 1995 Census Test: The Contamination Study" (1996). Because the Census Bureau planned to conduct NRFU on a sample basis for all blocks except those blocks that were to be included in the PES, where NFRU would be conducted on a 100 percent basis. If there was any sampling bias due to the nonresponse sampling, this bias could differentially affected the Integrated Coverage Measurement and the non-Integrated Coverage Measurement blocks. The Census Bureau evaluated this possibility and did not detect any contamination. In any case, sampling for NRFU was not used.

With respect to possible contamination, the A.C.E. is, with one exception, quite similar to the 1990 PES. In both surveys, housing unit listing was conducted before census mailout and NRFU. Personal visit interviewing was, in both cases, conducted after the end of NRFU, but concurrently with various census coverage follow-up field interviews.

One possible cause of contamination in the A.C.E. was that approximately one third of the A.C.E. interviews were conducted by telephone concurrent with census NRFU. The telephone interviews were restricted to cases that had a completed census questionnaire that provided a telephone number and excluded units in small multi-units structures and units without house-number-street-name. It is possible that some of these cases might have been visited later during NRFU, and that their responses to that operation were influenced by the A.C.E. interview.

We have not been able to detect serious contamination since moving away from the 1980 design. The ESCAP analysis of possible contamination was restricted to reviewing any available reports. The only report of possible contamination was from a Government Accounting Office debriefing. They reported that they were told by a few A.C.E. interviewers that the interviewers observed census personnel conducting CIFU. Contamination could have occurred, although no actual sharing of information was reported. The 1990 PES was also run concurrently with CIFU.

Next, we turn to inconsistency in post-stratification between the A.C.E. and the census. Some individuals may be classified in the initial census into different post-strata than in the P-sample. The initial census will certainly misclassify some individuals, causing them to be included in the wrong category. For example, some Hispanics may be classified as non-Hispanic, or some American Indians as White. To the extent that the coverage probabilities are equal only for the correct characteristics, census mis-classification (that is, incorrect post-stratification) may introduce correlation bias and synthetic error.

The introduction of multiple race reporting in both the census and the A.C.E. raised concerns about this type of error.

The impact of inconsistent post-stratification is a function of the proportion of misclassified records and the differences in coverage rates between the two post-strata. If only a few records are inconsistently classified, there will be little impact. Further, there is little impact on coverage if the misclassifications occur between post-strata with similar census coverage rates. Misclassification will only affect the quality of the estimates if there are large inconsistencies between post-strata with highly differential coverage rates.

One must note that inconsistent misclassification is not possible for all A.C.E. postratification variables. Region, metropolitan area size, type of enumeration area, and census mail return rate are all measured at the block level and are inevitably assigned the same value in both the P and E-samples. Inconsistent classification is only possible for the race/ethnicity, owner/renter, and age/sex domains.

We studied the differences in post-stratification for those people matched between the A.C.E. and the initial census. By assuming that these patterns apply equally to missed people and by working with the observed (estimated) coverage rates, we assessed the impact of these inconsistencies on the coverage estimates. Of course, this analysis took into account both the directly reported characteristics and the imputed characteristics in both the initial census and the A.C.E.

Of the two tenure groups, seven age/sex groups, and seven race/Hispanic origin domains, two groups stand out with particularly high rates of inconsistency: the domains of American Indian off reservation and Native Hawaiian and Pacific Islander. Both of these domains were new for 2000. In 1990 the American Indian off reservation population was combined with the non-Hispanic White and Other while the Native Hawaiian and Pacific Islander population was combined with the Asian population. The effect of the inconsistency for the American Indian off reservation is to push the resulting estimates toward that of the non-Hispanic White and Other population.

Another concern has been the treatment in the DSE of the cases involved in the Housing Unit Unduplication Operation (HUDO). As noted earlier, 1,019,057 housing units were analyzed during the HUDO and later re-instated into the census files. These units included 2,366,140 person records (including census imputations as well as data defined records). These records are referred to as "late census adds." These records were not included in the A.C.E. matching, processing, or follow-up processes. They were also excluded from the DSE. It is possible that, had these records been included in the A.C.E. and the DSE, the estimated undercount would have differed. To understand this difference, one must consider several factors.

Any of these person records that were not data defined would not have been included in the DSE in any case. Excluding them as late census adds rather than as whole person imputations makes no difference on the final DSE. Some of these person records were duplicates or other erroneous enumerations. Had they been included, the A.C.E. would have sampled and processed them and estimated the level of erroneous enumerations. Excluding these records from the DSE should reduce sampling error since sampling is no longer involved. It is possible that excluding them affected the nonsampling error. For example, it is possible that some of these cases might have been mis-coded had they been included. Further, given the way these were excluded and reinstated, it is possible that this process could have affected duplicate search or targeted surrounding block search. We were not able to quantify the nonsampling effect.

Some of these cases were, of course, correct enumerations. Including them in the A.C.E. would have had two effects. First, this would have raised the number of estimated correct data-defined enumerations. Second, it would have raised the number of matches from the P-sample to the census, since some of these people would have been included in the A.C.E. P-sample. If the ratio of matches to correct enumerations in the excluded cases is the same as the ratio matches to correct enumeration is the included cases, the DSE expected value should be nearly the same. However, if the people referred to in these correct cases were either much more likely to have been included in the A.C.E. or much less likely to have been included, then excluding these cases from the A.C.E. would have changed the level of correlation bias and affected the A.C.E. We have no reason to believe this to be the case. Finally, excluding these cases would have affected the sampling variances, especially if they were clustered. This effect, however, should be fully accounted for in the reported sampling error.

Finally, if these late census adds included geographic clustering of erroneous enumerations, they would increase the geographic heterogeneity in the census net undercount. Geographic clustering in net undercount that is not correlated with the A.C.E. poststratification variables will not be corrected by the A.C.E.

In 1990, the effect of late census adds on the DSE was studied and evaluated. Based on the 1990 experience, the treatment of the late census adds in the 2000 DSE was specified based on the theory noted above. In short, one cannot compare or project the effect of the late census adds in 1990 to the effect in 2000.

A related issue concerns the census whole person imputations. These are cases included in the census counts that are not data defined. These include three groups: cases where the number of people in the housing unit had to be estimated, cases where the number of people was known but all the characteristics of the household had to be estimated, and cases where there was a person reported on the questionnaire but with so little data that the census substituted the characteristics of another person. There were 5,691,184 whole person imputations in Census 2000 as opposed to 2,195,716 in the 1990 census. So while the A.C.E. design anticipated whole person imputations, the level was greatly increased.

Again, the effect of these whole person imputations will depend upon several factors. Some of these will be erroneous. For example, they may impute people into a unit that was vacant on Census Day or into a group of seasonal vacant units. Such imputations will, obviously, decrease the net undercount rate and could lead to an overcount. However, they should not affect the DSE in any way. However, if the imputation was, indeed, correct, then there were people living in the unit on April 1 who were not elsewhere counted, that is, not included in a duplicate housing unit. Had these people been included in the census, then some of them would have matched. Therefore, the number of census correct data-defined enumerations would have increased and the number of matches would have increased. If the ratio of matches to correct enumerations in the "imputed" cases is the same as the ratio matches to correct enumeration in the "non-imputed" cases, the DSE expected value should be the same. However, if people living in these units were either much more likely to have been included in the A.C.E. or much less likely to have been included, then imputing these cases (rather than enumerating them) would have changed the level of correlation bias and affected the A.C.E. Finally, the increased level of imputation would have affected the sampling variances, especially if they were clustered. This effect, however, should be fully accounted for in the reported sampling error.

Again, if incorrectly imputed cases were geographically clustered, they would increase the geographic heterogeneity in the census net undercount. Geographic clustering in net undercount that is not correlated with the A.C.E. poststratification variables will not be corrected by the A.C.E.

Synthesizing A.C.E. Quality

Comparison With Demographic Analysis and Demographic Estimates

Conclusions from this section

The disagreement between the results of demographic analysis and the A.C.E. removes an important independent verification of A.C.E. results. In 1990, demographic analysis clearly demonstrated that an adjustment based on the PES would have been conservative, that is, the true population would almost certainly have been higher still. In 2000, demographic analysis presents no such support, leaving the possibility that the A.C.E. would "over adjust."

Analysis reports important to this section

- Report B-4: "Accuracy and Coverage Evaluation Survey: Demographic Analysis Results," by J. Gregory Robinson
- Report B-16: "Demographic Full Count Review: 100 percent Data Files and Products," by Michael J. Batutis, Jr.

Discussion

demographic analysis has long provided the standard against which census accuracy is measured. See, for example, Committee on National Statistics, "Modernizing the U.S. Census" (1995) ("Demographic estimates are the primary means for comparing coverage for censuses over time for the nation as a whole."). Indeed, when people discuss the "steady improvement in census accuracy" or say that the 1990 census was the "first to be less accurate than its predecessor," they are using demographic analysis as a benchmark. See, for example, the "Report to Congress – The Plan for Census 2000" (1997, p. i), and Darga, "Sampling and the Census"(1999, page 14-15).

Demographic analysis, as the term is usually used, is the construction of an estimate of the "true" population using birth, death, migration and other data sources independent from either the current census or the A.C.E. demographic analysis provides independent measures of the net undercount by age, sex, and Black/non-Black. It represents a generally accepted historic data series, although, of course, it is subject to its own limitations and uncertainties. Among demographic analysis's important limitations is the lack of an historical data series to independently estimate the Hispanic, Asian, or American Indian populations. In addition, the level of emigration and undocumented immigration must be estimated using indirect methods. These limitations and uncertainty are documented in Robinson (1993), and Himes and Clogg (1992), as well as in the 1990 "D" studies.

Due to the uncertainty in the estimates of undocumented immigration, DA in 2000 uses a high and low range for making comparisons to the census and A.C.E. results. The "base" DA set of estimates include the current estimate of undocumented immigrants entering during the 1990's (2.76 million); the "alternative" DA set increases the DA estimate by doubling the assumed net flow of undocumented immigrants in the 1990's (5.52 million). The A.C.E. measures a net undercount of 3.3 million, or 1.15 percent for Census 2000. DA measures a lower net undercount than the A.C.E., according to either of the two sets of DA estimates developed. The "base" DA set estimates a net overcount of 1.8 million, or -0.67 percent in 2000. The "alternative" set, which increases the DA estimate to allow for additional undocumented immigration in the 1990's, gives a net undercount of 0.9 million, or 0.32 percent. The DA and A.C.E. estimates both measure a reduction in the net undercount in Census 2000 compared to 1990, but DA implies a greater change. Under the base set, the estimated DA net undercount rate fell by 2.5 percentage points from 1.85 percent net undercount in 1990 to -0.65 percent net undercount in 2000.

Further, the comparison of census counts to auxiliary data sets (such as school enrollment data for children and Medicare enrollment for the population 65 and older) are consistent in indicating Census 2000 is more complete relative to 1990. Both DA and A.C.E. measure a reduction in the net undercount rates of Black and non-Black children (ages 0-17) compared to 1990. Both methods also measure a reduction in the net undercount rates of Black men and women (ages 18+). DA finds a reduction in the net undercount rates of non-Black men and women in Census 2000 compared to the rates of previous censuses. The reduction is large under the base DA set and moderate under the alternative DA set. The A.C.E. indicates no change or a slight increase in undercount rates for non-Black adults as a group.

The A.C.E. sex ratios (ratio of males per 100 females) for Black adults are much lower than DA "expected" sex ratios, implying that A.C.E. is not capturing the high undercount rates of Black men relative to Black women (the well-known "correlation bias"). The size of this bias is about the same as in the 1990 PES.

A more recent complication warrants mention. The demographic analysis method requires reconciling the reporting of race in the vital statistics system with race as reported in the census. For example, in the birth registration system the race of the mother and the father are reported, rather than the race of the child. For the first time, the census questionnaire instruction was to "mark one or more races." This change introduces a new consideration into the reconciliation of reported race data. Depending on the treatment of people who report Black and at least one other race, the Black undercount estimate ranges from 0.9 percent to 4.7 percent. However, in either case a clear differential undercount between the Black and non-Black population is evident, ranging from at least 1.8 percent to perhaps 6.2 percent.

If we take DA as representing a reasonable low estimate of population in 2000, what would represent a reasonable high? Although we tried several different scenarios raising undocumented immigration, for purposes of simplicity, we have assumed a doubling of the undocumented population for our alternative demographic assumption. Doubling undocumented immigration would result in an alternative DA that implies a percent foreign-born of 11.13 (compared to 10.61 in the unweighted CPS) and a percent Hispanic of 12.72 (compared to 12.55 in the unadjusted Census 20000 results). Until we can get a fuller set of data from Census 2000 to recalibrate the DA estimates in detail, this alternative would seem to be a reasonable upper bound on the total number of undocumented immigration in the 1990s.

The demographic analysis estimates may have underestimated the population and, thus, the net undercount, in 1990. Indeed, Robinson et al. (1993, p. 1070) states that "the demographic net undercount estimates are biased in that they may underestimate the "true" net undercount" (See also 1990 DA Evaluation Project D-10). Thus in 1990, the DA production or preferred estimate is as a net undercount of 4.55 million. Analysis showed that it was very unlikely that the true undercount was less than 4 million. This showed that the 1990 PES almost certainly did not overestimate the net national undercount. However, the upper range of the demographic analysis uncertainty estimates was a net undercount of over eight million people. Indeed, the midpoint of the range (6.2 million) is higher than the 1990 demographic analysis production estimates. (Estimates are based on Robinson 1993, Table 4 times 249 million).

It is important to note that errors in estimating the 1990 population will affect a comparison with the A.C.E. with respect to the level and pattern of the undercount. It will not affect the measured change between censuses. The internal consistency of the demographic estimates permits trends and changes in the coverage pattern over time to be estimated more accurately than the exact level of net coverage in any given census. (Report B-4, "Demographic Analysis Results")

Historically, demographic analysis's important strength has been its ability to measure sex ratios accurately. While inconsistency in reporting racial data may introduce uncertainty into the demographic analysis estimates of a specific population group, in many instances the inconsistency will affect both sexes equally, so that the inconsistency's effect on the expected sex ratio should be quite small. In 1990, many of the comparisons between the initial census, the PES, and demographic analysis centered on the sex ratios.

Post-Enumeration Survey - A.C.E. error of closure

The estimated population from the 1990 dual system estimates based on the PES can be projected forward and compared to the estimated population from the 2000 dual system estimates based on the A.C.E.. To the extent that the population change during the decade is well estimated, the difference must be attributable to changes in the level and patterns of errors in the two dual system estimates. The following table is instructive:

Table 5 – A comparison of the 1990 PES total population with the A.C.E. accounting for population change.

	Base Demographic Analysis Estimates	Alternative Demographic Analysis Estimates
1990 Post-Enumeration Survey Dual System Estimates	252,756,428	252,756,428
Natural Growth	17,331,261	17,331,261
Legal Immigration	9,266,974	9,266,974
Emigration	2,652,597	2,652,597
Undocumented Immigration	2,765,196	5,530,392
"Expected 2000 Population"	279,467,262	282,332,458
2000 Accuracy and Coverage Evaluation Survey Dual System Estimates	284,683,782	284,683,782
Error of Closure	5,216,520	2,451,324
Error of Closure Percent	1.9 percent	0.9 percent

Estimates are for the total population, including populations excluded from the 1990 and 2000 Dual System Estimation estimates.

Unless the demographic analysis estimates of change are inaccurate, it is clear from this table that the error level of the 1990 PES DSE must differ from that of the 2000 A.C.E.. There are several possible causes. Assuming change is correctly measured, the difference between the 1990 PES carried forward and the 2000 A.C.E. must be due to either sampling or non-sampling errors in the PES or A.C.E. Further, to account for differences beyond sampling error, one must assume that the non-sampling error levels were different in the two surveys.

The A.C.E. universe differed from that of the PES. The A.C.E. excluded the noninstitutional nonmilitary group quarters, while the 1990 PES had included this group. The A.C.E. DSE implicitly attributes zero coverage error to this group. The PES DSE attempted to measure the coverage of this group. However, there is no evidence that this coverage of this group was so very far from correct as to explain much of the PES/A.C.E. error of closure.

Another explanation would be that the 1990 PES DSE had much higher levels of correlation bias overall than did the A.C.E.. It is certainly possible, even likely, that the 1990 PES underestimated the net undercount. This is the implication of any comparisons with the 1990 demographic analysis estimates and is reinforced by comparing the 1990 PES net undercount (1.65 percent) the range of uncertainty surrounding the 1990 demographic estimates (1.63 percent to 3.36 percent).

Noting that the 1990 PES most certainly was affected by correlation bias and almost certainly underestimated the net national undercount does not, however, explain the change between censuses. One possibility is that the improved publicity campaign and improved community outreach surrounding the census may indeed have persuaded people to participate in the census (both initial and A.C.E. phases) while they, or at least similarly situated people, did not participate in 1990. It must be noted that at this point, this explanation remains no more than an interesting hypothesis.

The analysis of errors in the 1990 PES indicated that except for correlation bias, the other errors tended to increase the estimated population. That is, corrections for the bias would lower the estimates. Thus, to explain the error of closure one must posit that the errors in the A.C.E. were considerably higher than those in the 1990 PES.

However, the analyses of the 2000 A.C.E. seems to indicate that the errors were better controlled and probably smaller in 2000 than they were in 1990. The one exception noted above is errors from coding an E-sample case as correct when, in fact, it was physically located outside the search area and should have been coded as erroneous. We have documented that this occurred, but not to the scale indicated by the error of closure. Since this kind of error was also present in the 1990 census, one must assume a large increase in this or some other positive error to explain the error of closure.

A fundamental assumption of the loss function analysis conducted in connection with the A.C.E. and discussed below is that the pattern of errors for the A.C.E. is similar to the pattern measured in the PES. If the error level or structure of the A.C.E. differs substantially from that of the PES, then findings from the loss function analysis are far less certain.

Comparing the accuracy of the A.C.E. to the accuracy of the uncorrected census

Conclusions for this section

Analysis shows that if one assumes that A.C.E. processing errors are assumed at or near the level measured in 1990 and assumes that there is little or no correlation bias, then either the unadjusted census is more accurate or the two are of nearly equal accuracy. If one assumes that the A.C.E. processing errors have been greatly reduced or if moderate or substantial correlation bias is present, then the A.C.E. adjusted results are more accurate, often by a large margin. Allowing for synthetic error does not reverse these findings. However, these findings are dependent on the assumption of similar pattern of errors as was measured in 1990. If this assumption is not valid, no conclusions can be drawn.

Analysis reports important to this section

- Report B-13: "Accuracy and Coverage Evaluation Survey: Comparing Accuracy" by Mary H. Mulry and Alfredo Navarro
- Report B-14: "Accuracy and Coverage Evaluation Survey: Assessment of Synthetic Assumptions" by Donald J. Malec and Richard A. Griffin

Discussion

Knowing the level of error in the A.C.E. is not enough because the A.C.E. decision will not be made in a vacuum; rather the A.C.E. will be compared to the unadjusted census to determine which is more accurate for redistricting purposes. Both the adjusted and the unadjusted data sets will have their own patterns of error.

As discussed at length in the June 2000 "Accuracy and Coverage Evaluation: Statement on the Feasibility of Using Statistical Methods to Improve the Accuracy of Census 2000," there are several important criteria in assessing accuracy. For purposes of the ESCAP decision, the Census Bureau has evaluated both numeric and distributive accuracy. Both types of accuracy are important criteria for numbers that will be used in the redistricting process, and both types of accuracy have independent importance as tools in assessing A.C.E. and census quality. Additionally, as discussed in the above document, accuracy can be measured at different geographic levels.

Another way to measure overall accuracy is to prepare Loss Functions. Mean squared error is a form of loss function. The Census Bureau prepared Loss Function Analyses in connection with the 1990 adjustment decision and also in connection with the 1993 decision regarding use of adjusted data as a base for the intercensal estimates. These Loss Functions were able to account for estimated bias in the PES estimates. The accuracy criteria discussed above guided our design of the loss functions. We prepared loss functions to determine the comparative accuracy of the adjusted and unadjusted data sets at the state and Congressional district levels, to measure both numeric and distributive accuracy.

The 1990 studies and subsequent analyses addressed this issue though complex simulation procedures (See, P-16, as well as Mulry and Spencer [1993]). The Census Bureau concluded that adjustment of the 1990 census would have improved distributive accuracy for states and for areas with populations of more than 100,000. Later Census Bureau work revealed that in general one could not distinguish an improvement in distributive sub-state accuracy for areas with populations of less than 100,000 (Obenski and Fay, 2000).

The Loss Function Analyses that we conducted to inform the ESCAP decision should not be considered determinative for several reasons. Although A.C.E. variances are available, complete information on A.C.E. biases is not. Accurate bias data are a vital component of any Loss Function. For the purpose of ascertaining preliminary Loss Function information to guide the ESCAP decision, therefore, the Census Bureau assumed that the bias in the A.C.E. was similar to biases in the 1990 PES. To some extent, the PES biases were modified based on an analysis of differences in the PES and the A.C.E., but the extent of this analysis was limited. Finally, one should keep in mind that more complete Loss Functions will be prepared as part of the final evaluation process, many months after the ESCAP recommendation. These more complete Loss Functions, performed after more data are available, may well reach results different from those of the preliminary Loss Functions.

Although several loss functions were computed, three are of principal importance.

The Weighted Squared Error Loss for all levels is a measure of numeric accuracy. For example, it treats a 1 percent error in estimating the population total for a state proportional to the state's size. If state A is twice the size as state B, then a 1 percent error in estimating the size of state A is considered twice as serious.

The Weighted Squared Error Loss for shares is a measure of proportional or share accuracy. It treats a 1 percent (not percentage point) error in the share of a state proportional to that state's size. If state A is twice the size of state B, that is, state comprises 2 percent of the nation's population while state B is 1 percent, then a 1 percent error in estimating state A's share of the national population is weighted at twice the error as a 1 percent error in estimating state B's share.

Equal Congressional District Squared Error Loss is a measure of within state share accuracy closely related to state congressional redistricting . This measure only looks at shares within state. The shares are computed on the current congressional districts, and errors from the census and A.C.E. are estimated. Errors within state shares are then summed over the fifty states to produce a national index of relative accuracy.

For each measure of accuracy, we computed the relative loss. This is a measure or estimate of how the census and A.C.E. losses compare. It is computed as the Census Loss divided by the A.C.E. loss. Relative loss of less than one indicates that, for that measure and those assumptions, the census is estimated to be more accurate.

To estimate the relative accuracy for the census and the A.C.E., one must properly account for several things. First is the estimated levels of undercount in the census as measured by the A.C.E. Second is the sampling variance in the A.C.E.. Third is the level of bias present in the A.C.E. As we had no direct measures of the level of bias in the A.C.E. (Except for ratio and correlation bias), we assumed the level measured in the 1990 PES. The analysis also took into account the variance of the estimated biases in 1990. See B-13 and B-19 for a full description.

Models were run using many variations on the assumptions, which are documented in B-13. Most important to the results are the following assumptions.

- The Level of A.C.E. processing error: This includes A.C.E. matching error and E-sample coding error: One hundred percent error indicates that there was no improvement from the 1990 measured levels.

- The level of correlation bias for adult men: Zero correlation bias indicates that there is not allowance for this bias. One hundred percent indicates that the full level of correlation bias for adult men implied by the demographic analysis sex ratios. In addition, some runs were made assuming that the correlation bias for Hispanics was at the same level measured for Blacks.

- The 2000 estimated undercounts and their estimated sampling variances were used. All other A.C.E. biases were assumed at their 1990 levels, including an allowance for the variances on estimating these in 1990.

Some principle findings are summarized in Table 6:

Table 6: Relative Loss by Degree of Processing Error and Correlation Bias

Model	Degree of Correlation Bias	Degree of Processing Error	Census Loss / A.C.E. Loss (St. Levels)	Census Loss / A.C.E. Loss (St. Shares)	Census Loss / A.C.E. Loss (CD shares)
NA	0%	100%	0.519	1.783	0.995
1	100%	0%	17.488	1.125	2.068
1	100%	25%	18.565	1.318	1.975
1	100%	50%	14.108	1.500	1.870
1	100%	75%	8.242	1.656	1.759
1	100%	100%	4.413	1.780	1.651
2	10%	90%	0.770	1.761	1.147
2	20%	90%	0.897	1.792	1.265
2	50%	90%	1.416	1.838	1.554
2	75%	90%	2.048	1.821	1.688

Model 1 - correlation bias is present for males except for Non-black males age 18 to 29.
Model 2 - correlation bias is present for Black males only.
States use weighted squared error loss and congressional districts use equal CD squared error loss.

The reader can see that if one assumes no reduction in processing error over 1990 as well as little or no correlation bias, the census is as accurate or more accurate than the adjusted A.C.E. for state levels, less accurate for state shares, and about as accurate for CD shares. This clearly demonstrates how sensitive the results are to the model assumptions. As noted above, an analysis of the Error of Closure between the A.C.E. and the PES indicates that the patten and level of error of A.C.E. may not necessarily follow that found in the PES. Therefore, the results of the loss functions must be interpreted cautiously. If the assumptions of similar patterns of errors do not hold even approximately, no direct conclusion can be drawn.

To assess the impact of synthetic error (local heterogeneity in the unadjusted census results) on comparison between Census and A.C.E. relative accuracy, several models were run including both the local heterogeneity and the assumed level of bias in the A.C.E.. (B-14, "Assessment of Synthetic Assumptions"). These analysis indicated gains in accuracy from adjustment even accounting for synthetic bias. However, these results are subject to the same limitations noted above.

The loss functions run for counties with populations below 100,000 indicated that the unadjusted census was more accurate regardless of the level of correlation bias assumed. This caused some concern, since this was not the case for the 1990 census adjustment. One should remember, however, that counties below 100,000 are not the same or even representative of all areas of less than 100,000. However, the analysis found that the adjustment was more accurate when considered in terms of all counties for both numeric and distributive accuracy.

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B-6, "Accuracy and Coverage Evaluation Survey: Person Matching and Follow-up Results" by Danny R. Childers, Rosemary L. Byrne, Tamara S. Adams, and Roxanne Feldpausch

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B-9, "Accuracy and Coverage Evaluation Survey: Dual System Estimation Results" by Peter P. Davis

B-10, "Accuracy and Coverage Evaluation Survey: Consistency of Post-Stratification Variables" by James Farber

B-11, "Accuracy and Coverage Evaluation Survey: Variance Estimates by Size of Geographic Area" by Michael D. Starsinic, Charles D. Sissel, and Mark E. Asiala

B-12, "Accuracy and Coverage Evaluation Survey: Correlation Bias" by William R. Bell

B-13, "Accuracy and Coverage Evaluation Survey: Comparing Accuracy" by Mary H. Mulry and Alfredo Navarro

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P-5, "Analysis of P-Sample Fabrications from PES Quality Control Data"
P-5a, "Analysis of Fabrications from Evaluation Follow-up Data"
P-6, "Fabrication in the P-sample - Interviewer Effect"
P-7, "Estimates of P-Sample Clerical Matching Error from a Rematching Evaluation"
P-10, "Measurement of the Census Erroneous Enumeration Clerical Error made in the Assignment of Enumeration Status."
P-11, "Balancing Error Evaluation"
P-13, "Use of Alternative Dual System Estimators to Measure Correlation Bias"
P-16, "Total Error in PES Estimates for Evaluation Post Strata"

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Table A-1: Census 2000 A.C.E. 64 Post-Stratum Groups by region - Percent Late Adds

Race/Hispanic Origin Domain Number*	Tenure	MSA/TEA	High Return Rate NE	High Return Rate MW	High Return Rate S	High Return Rate W	Low Return Rate NE	Low Return Rate MW	Low Return Rate S	Low Return Rate W
Domain 7 (Non-Hispanic White or "Some other race")	Owner	Large MSA MO/MB	0 5	0 2	0 2	0 3	2 1	1 4	0 3	0 5
		Medium MSA MO/MB	0 4	0.2	0 3	0 3	1 0	0 6	0 4	0 4
		Small MSA & Non-MSA MO/MB	0.4	0 3	0 3	0 3	0 8	0 4	0 4	0 5
		All Other TEAs	1.3	1 3	1 6	1 4	1 9	1.9	2 3	2 2
	Non-Owner	Large MSA MO/MB	1.2	0 8	0 5	0 5	1 7	1 6	0 7	0.6
		Medium MSA MO/MB	1.3	0 7	0.6	0 6	1 8	0.9	0 6	0 6
		Small MSA & Non-MSA MO/MB	1 3	0 7	0 7	0 7	1 5	0.7	0 7	0.7
		All Other TEAs	3.2	2 2	2 4	2 7	3.8	2.5	2 8	3 5
Domain 4 (Non-Hispanic Black)	Owner	Large MSA MO/MB	0 8	0 6	0 3	0.3	3 2	1.7	0 4	0.3
		Medium MSA MO/MB								
		Small MSA & Non-MSA MO/MB	1 0	0 4	1 3	0.7	0 8	0 7	2 3	1.4
		All Other TEAs								
	Non-Owner	Large MSA MO/MB	0.9	1.0	0 6	0 5	1.6	1 7	0 8	0.6
		Medium MSA MO/MB								
		Small MSA & Non-MSA MO/MB	1.5	0 6	1 2	0.7	1 9	0.6	1 7	1 2
		All Other TEAs								
Domain 3 (Hispanic)	Owner	Large MSA MO/MB	1 2	0.5	0 4	0 4	3 3	2.7	0 6	0 5
		Medium MSA MO/MB								
		Small MSA & Non-MSA MO/MB	1.1	0 6	1 2	0 9	1 2	1.1	2 2	2.4
		All Other TEAs								
	Non-Owner	Large MSA MO/MB	1 2	1 0	0 6	0 5	1 9	2.2	0 5	0.6
		Medium MSA MO/MB								
		Small MSA & Non-MSA MO/MB	2.1	1 0	1 4	1.4	1 7	0.9	2 1	3.3
		All Other TEAs								
Domain 5 (Native Hawaiian or Pacific Islander)	Owner		1.6		0 5		0.4		0 9	
	Non-Owner		1 2		0.6		0 6		1 0	
Domain 6 (Non-Hispanic Asian)	Owner		1 8		0.5		0 4		0 5	
	Non-Owner		1 3		0 8		0 5		0 6	
American Indian or Alaska Native — **Domain 1** (On Reservation)	Owner		1 6		0.8		1 5		0 9	
	Non-Owner		0 8		0 7		1 4		1 1	
American Indian or Alaska Native — **Domain 2** (Off Reservation)	Owner		1 4		0 7		1.5		0 9	
	Non-Owner		1 7		1 0		1 3		1 0	

• For Census 2000, persons can self-identify with more than one race group For post-stratification purposes, persons are included in a single Race/Hispanic Origin Domain This classification does not change a person's actual response. Further, all official tabulations are based on actual responses to the census

Table A-2: Census 2000 A.C.E. 64 Post-Stratum Groups by region - Percent iis

Race/Hispanic Origin Domain Number*	Tenure	MSA/TEA	High Return Rate NE	High Return Rate MW	High Return Rate S	High Return Rate W	Low Return Rate NE	Low Return Rate MW	Low Return Rate S	Low Return Rate W
Domain 7 (Non-Hispanic White or "Some other race")	Owner	Large MSA MO/MB	0 9	0 9	1 0	1.1	2.6	2 6	1 8	1 8
		Medium MSA MO/MB	0 8	0 8	1 1	1 2	1.8	1.6	1 6	1 9
		Small MSA & Non-MSA MO/MB	0 8	0 9	0 9	1 1	1 2	1 8	1 5	1 9
		All Other TEAs	1 6	1 1	1 1	2.8	1.6	1 5	1 5	2 5
	Non-Owner	Large MSA MO/MB	2.1	2 0	2 7	1.9	4 4	5 4	3 9	3 1
		Medium MSA MO/MB	1 6	1 6	2 4	2.3	3 5	3 0	3.4	3 2
		Small MSA & Non-MSA MO/MB	1.4	1.6	2 0	1.9	2 2	3 3	2 8	2 8
		All Other TEAs	2 1	1 6	1 8	3.7	3 0	2 1	2 5	3 9
Domain 4 (Non-Hispanic Black)	Owner	Large MSA MO/MB	2.7	2 4	2 4	2.5	5 3	4 9	3.3	3 4
		Medium MSA MO/MB								
		Small MSA & Non-MSA MO/MB	2.6	2 3	2 2	2.9	2.8	3 4	3 3	5 2
		All Other TEAs								
	Non-Owner	Large MSA MO/MB	4.4	3 3	3.5	3.1	6.6	6.0	4 4	4 5
		Medium MSA MO/MB								
		Small MSA & Non-MSA MO/MB	2 5	3 0	2 9	3.2	3 7	5 9	3 9	5 6
		All Other TEAs								
Domain 3 (Hispanic)	Owner	Large MSA MO/MB	2.5	2 5	2.6	4.0	5.9	5.3	4.2	5 0
		Medium MSA MO/MB								
		Small MSA & Non-MSA MO/MB	2 4	2 3	4 0	3 9	3 8	3 5	4 1	4 5
		All Other TEAs								
	Non-Owner	Large MSA MO/MB	3 6	3.2	3 2	3.8	5 4	5.7	4 0	4 3
		Medium MSA MO/MB								
		Small MSA & Non-MSA MO/MB	2 5	2.8	3 7	4 1	3.9	4.0	4 9	5.4
		All Other TEAs								
Domain 5 (Native Hawaiian or Pacific Islander)	Owner		3 3		3.7		2 8		3 7	
	Non-Owner		5 1		4.3		4 3		3 6	
Domain 6 (Non-Hispanic Asian)	Owner		2 8		2.3		2 2		2 4	
	Non-Owner		3 8		3.3		3 3		3 1	
American Indian or Alaska Native — **Domain 1** (On Reservation)	Owner		8 8		4.9		4 2		5 0	
	Non-Owner		6.7		4.7		3 7		4 6	
American Indian or Alaska Native — **Domain 2** (Off Reservation)	Owner		2.7		2.1		1.9		3 0	
	Non-Owner		3 6		2.7		2 7		3 2	

- For Census 2000, persons can self-identify with more than one race group For post-stratification purposes, persons are included in a single Race/Hispanic Origin Domain This classification does not change a person's actual response Further, all official tabulations are based on actual responses to the census.

Table A-3

Census 2000 Evaluations Program
Category Report Schedule

CATEGORY	AVAILABILITY OF CATEGORY REPORT
A: Response Rates & Behavior Analysis	Spring 2002
B: Content/Data Quality	Summer 2003
C: Data Products	Summer 2001
D: Partnership and Marketing Programs	Winter 2001
E: Special Populations	Winter 2001
F: Address List Development	Fall 2002
G: Field Recruiting & Management	Summer 2001
H: Field Operations	Winter 2002
I: Coverage Improvement	Winter 2002
J: Ethnographic Studies	Spring 2003
K: Data Capture	Fall 2002
L: Processing Systems	Winter 2002
M: Quality Assurance Evaluations	Spring 2003
N: Accuracy & Coverage Evaluation Survey Operations	Fall 2002
O: Coverage Evaluations of the Census & of A.C.E. Survey	Summer 2002
P: A.C.E. Survey Statistical Design & Estimation	Winter 2003
Q: Organization/Budget & MIS	Fall 2001
R: Automation of Census Processes	Summer 2001